OBITUARY NOTICE

SIR GODFREY THOMSON

1881–1955

Over Ottercap Hill they cam in
And so down by Rotherley Crag
Upon Greenleighton they lighted down.

Sir Godfrey's heart, it may be, rested in the hills and moors of Northumberland. One of his earliest papers was on the influencè of Northumberland's geography upon its history, in which he traced the paths of marauding Scots, and of the far-spreading Romans, upon his northern county. The Scots might taunt a Percy under the walls of Newcastle: but dead would be the Douglas at Otterburn. The stirrings of the border, it seems, found deep echoes in Sir Godfrey's life, his work, his personality. He loved regional things. There was a delight and pride, perhaps, in his belief that the hill stock of Northumberland was of superior quality, descended not from a peasantry, but from the bold 'reivers and raiders of the marches', the men of Rotherley, Carter Bar, and Greenleighton, 'who pitted their wits against the equally alert Scots across the border'. Little is ever heard of the raids of Northmen into the wilds of Roxburgh or beyond; but it may have been with some such thoughts that Godfrey Thomson left Newcastle for Moray House thirty years ago—taking with him, however, chivalry and keen intelligence, scholarliness and wisdom, to live and commune with the Scots as one of them.

There are others, I am sure, who are better qualified than I am to appraise the man and his work. But I knew him in some sort of a personal way for over thirty years, and heard about him before that when I was a boy. As Professor Thomson he was my first mentor, and I was perhaps the first of his students of Newcastle days to pursue some of his interests. There are other bonds connecting us, some academic, others inconsequential but warm for the soul, and these matters perhaps give me the privilege to write about him. The young Godfrey went to Rutherford College in Newcastle, and then studied mathematics and physics at Armstrong College. There was a common thread, to which he himself used to draw attention, between his early invention of a compound pendulum and his childhood fascination with clocks and watches, and between these matters, for which it was always possible to develop elegant mathematical expressions, and his later interest in sampling theories. The beating out of sine curves is perhaps not very different from the complex throbbing of a human brain. A single strand of motivation, therefore, seems to run through all Sir Godfrey's work, from his earliest paper on a pendulum, to his latest exquisite tailpieces on *The Geometry of Mental Measurement*. What the motive stemmed from no doubt lay deep, if not inquiet, in his heart. But what it fashioned is wide open for anyone to see. For it is given to very few to combine constructive judgement and brilliant exposition as completely as did Sir Godfrey. His *Factorial Analysis of Human Ability* is still by far the most readable account of factor analysis available to us; and to expound these intricate matters so cogently is, as Thackeray said of Macaulay, to read twenty books to write a single sentence, or to travel a hundred miles for a single line about a country's beauty. Sir Godfrey was always supremely lucid. His expositions are

crystal clear. But to this he added a fine eagerness for truth, and a sharp revolt against arbitrary assumptions. His Northumbrian forthrightness was never better put to service than in his famous stand against all arbitrary assumptions in Spearman's Theory of Two Factors. But for every critical probe there was always added an eagerness to acknowledge the merits of any theory, and a punctiliousness in giving credit where it was due. It was characteristic of the time, only thirty or so years ago, that these men could not converse freely with one another. The issues dividing Spearman and Thomson loomed too large, like monstrous ogres. Yet each, as I know, had enormous admiration for the other.

A full or balanced appraisal of Sir Godfrey's work is scarcely possible at this juncture. His early interests in physics led imperceptibly to empirical and theoretical work on the psychophysical methods, and thence to his notable chapters in *The Essentials of Mental Measurement*, for which he collaborated with William Brown. The *Essentials* is now of scholarly rather than immediate scientific interest, though the chapters on the sampling theory of ability represent Sir Godfrey at his most cogent and creative. His main point, made first by him I believe, was that innumerable factorial analyses are possible for data, and that any choice between them must be, as he said, on psychological grounds. Factors, he argued, are 'not necessarily anything real, they are only pointer-readings, mathematical symbols'. He agreed with Sir Cyril Burt in this matter. It is hard, indeed, to believe that anyone ever thought otherwise, but sophistication in this matter seems to have grown slowly, although prior to and independent of the increasing knowledge that is now available about the logic and philosophy of science. Factors, however, may be directly empirically rooted—they are then usually called 'descriptive' only; in this sense they are strictly abstract models of something real. Hypothetical factors of this kind are most directly employed in the structuring of samples, as in Q-method, and they are then indeed merely 'pointer-readings, mathematical symbols'. But there is a third, and perhaps a different, possibility for factors, which may have been Sir Godfrey's major concern. This stemmed from his belief that the mind is not divided into 'unitary' factors, but is a 'rich, comparatively undifferentiated complex of innumerable influences; on the physiological side an intricate network of possibilities of intercommunication'. The 'unitary' factors which he rejected, if they meant anything, must be of the theoretical kind, to which I have just referred. What, then, remained for Sir Godfrey? He found the answer in the work of Sir Cyril Burt, doyen of English factorists. In a review of Burt's *The Factors of the Mind* (*Brit. J. Educ. Psychol.*, 1941, XI, 46) Sir Godfrey expresses deep interest in Burt's comparison of factor analysis and the theory of groups used by theoretical physicists in quantum theory. The concern in the two cases is with the structure or patterns contained in action, with relationships or structure 'which exist between unknown and unknowable components'. Factors here merely specify systems of relations, without theoretical content of the kind we can build into Q-samples, or such as might be represented by lines of latitude and longitude on a map. What seems to have interested Thomson was the feeling, which he shared with Burt, that the final goal of factor analysis must be just such ultimate explanations, in terms of components which are perhaps unknown and unknowable. Sir Godfrey, however, liked the physiological model of networks of intercommunication in a brain; his sampling theory of intelligence represents this very well, and it may remain for generations as an acceptable and ultimate explanation of very complex matters indeed.

Sir Godfrey's knowledge of factor analysis was both minute and prodigious. But there

is also a more general side of his work, which is also statistical, which was no less thoroughly pursued. It shows best in his Chairmanship of the Scottish Mental Survey Committee, with its studies on intelligence and its ramifications into fertility, social class, occupational status, twinning, family position, test sophistication and the like. He brought to these matters wisdom and caution. The complexities of the community and family, clearly, matched those of the brain and its networks of intercommunication. Statistical sophistication was essential to both. In these two preoccupations, then, one a sampling theory of intelligence, physiologically conceived, and the other the statistical analysis of complex sociological matters, we can see both the great strength and the great weakness of Sir Godfrey's work. His strength was to bring fine scholarship and mathematical rigour to bear upon these statistical matters. His influence on Ferguson, Fraser Roberts, Emmett, Lawley, Ledermann and many others is a rich testimony to this strength, and something about which the University at Edinburgh cannot but be proud. The weakness, so it seems to me, was that in between the intricacies of the brain on the one hand, and of the sociological matters on the other, human behaviour as such seemed somehow to be overlooked. But science is long, and life short, and everyone has a choice to make amongst many possibilities. Sir Godfrey, like Florence Nightingale long before him, was a 'passionate statistician'. He clearly held, with Quetelet, that 'we can judge of the perfection to which a science has come by the facility, more or less great, with which it may be approached by calculation'.

Of Sir Godfrey's great gifts as a teacher others have spoken. His written word was no less brilliantly espoused. 'No more than a bird can fly with one wing, or a man breathe only by inhaling' is a typical allusion from his *Instinct, Intelligence, and Character*, on some fallacies in reasoning. And as a chairman of meetings he was surely incomparable. When, as a student, I bumped into him on a stairway at Armstrong College—he was with his own sage mentor, Professor Mark Wright—his bearing was somewhat military, his personality a little, shall we say, superior or haughty. The perception of him was no doubt jaundiced, as any student's is apt to be. But I think that I discerned, over the years, a profound mellowing and growth of personality in the man. I saw him last seven years ago, and I felt then, as I do now, that one could say about him what was written about Washington Irving: 'in his family gentle, generous, good-humoured, self-denying, affectionate; in society an example of complete gentlemanhood; unspoiled by prosperity; never obsequious to the great; eager to acknowledge every contemporary's merit; always kind and affable to members of his calling; a charming master of language....' Such was the Godfrey Thomson I knew from a distance, for a long time.

W. STEPHENSON

A COMPARISON OF THE PERSONALITY PROFILE (16 P.F.) OF EMINENT RESEARCHERS WITH THAT OF EMINENT TEACHERS AND ADMINISTRATORS, AND OF THE GENERAL POPULATION

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I. The setting of the problem

Research on research is a recent conception, but it has come none too soon. As the investigations of Wolfe (1952) indicate, the well-springs of professional talent never flow too freely, and of all professional groups that of creative researchers is probably the most uncertain in its supplies of talent. Nevertheless, because of the importance of this small band to continued social prosperity and scientific advance, its occupational selection and conditions of working creatively need to be studied, despite their complexity, as soon as understanding of personality measurement and the mental processes of creativity (Kretschmer, 1931; Spearman, 1933; Guilford, Wilson & Christensen 1952), make possible an improvement upon present practices.

Probably more can be contributed at this moment through selection than merely by multiplying hours spent on research. Swelling research endowments without increasing our power to select researchers may merely drown the light of understanding. And unless good researchers can be selected younger we have the sad alternatives (*a*) of waiting till a man has proved himself by his works, which permits him to begin life at forty, (*b*) of pouring endowments on anyone whose birth certificate proves him to be young, a despairing and wasteful procedure, but one which any research council is at times tempted to follow, or (*c*) of judging research by the design submitted, rather than by the person, a practice which makes the research world safe for all, including all rule-following mediocrity.

Three well-known attacks upon the problem, which have recently rallied interest, are those of Guilford *et al.* (1952) upon the nature of creativity, and of Roe (1953) and Van Zelst & Kerr (1954), upon the personality of the researcher. This article does not purpose to survey and integrate, but we may point out that our objective is substantially the same as that of Roe, though our results cannot be aligned, because Roe had to depend at the time of planning her research on the Rorschach and the T.A.T., whereas our measurement is directed to personality dimensions more recently factor-analytically investigated (Cattell, 1950*c*) and confirmed (Cattell, Day & Meeland, 1953; Cattell, 1955*a*) in the Sixteen Personality Factor Questionnaire (Cattell, 1955*b*).

While this measurement of primary personality dimensions has the advantage of greater reliability (including lesser subjectivity of scoring) than the Rorschach and T.A.T., and also of higher validity in the sense of accounting for more of the variance of more

demonstrably independent directions of personality variation, it has the disadvantage of all questionnaire techniques: possible motivational distortion and possible undue demands on literacy. Distortion has been avoided, as far as possible, in the 16 P.F. by indirection of questioning, but with persons eager for a particular job, some distortion undoubtedly remains for correction, and low educational level can also produce random error. It may be claimed, however, that the present investigation of teachers, administrators and researchers presents the most ideal situation for the use of the questionnaire, in that these subjects are educated people, appreciative of the scientific need for candour, secure in their positions, and assured of anonymity in their results.

Vocational selection and guidance depend in principle upon either or both of two statistical-conceptual systems. In one the personality-ability *profile* is determined for the typical member, by such devices as used by R. L. Thorndike (Thorndike & Norris, 1952), Cronbach & Gleser (1953) or one of the present writers (Cattell, 1949) in the formally identical problem of classifying culture patterns (Cattell, 1950*b*). Any individual propositus then has the similarity of his personal profile to this successful (or at least surviving) type determined by the pattern similarity coefficient, r_p, simple or weighted (Cattell, 1949; Cronbach & Gleser, 1953). In the other, a criterion of success within the occupational group is set up, and regressions, linear or curvilinear, of the various personality factors (including abilities) upon this criterion are worked out. Enlightened use of the values from either requires it to have the signature of the particular social supply-and-demand conditions obtaining at the time.

The present investigation was planned with the first of these objectives—to determine the personality profile of the typical researcher in relation to the average adult and the profile types of other occupations, especially of those for the related callings of scientific teachers and administrators, among which distinction most needs to be made in guiding students to careers. It seems to us that this delineation of the occupation *per se*, relative to others, should precede, logically and statistically, the investigation of the variance *within* the occupation. Besides, the preparation of criteria of success, involving, if it is to be effective, a good deal of time on the part of high-level professional people, was too expensive for the present, unendowed research.

II. Research plan and procedure

The testing instrument, having been frequently described in other investigations (Cattell, 1950*a*, *b*, 1955*a*; Cattell *et al.* 1953; Drevdahl, 1954; Shotwell & Cattell, 1954; Stice & Cattell, 1954) needs little comment, except to say that it has recently been re-factored and independently rotated with confirmation of all but one of the factor positions (Cattell, 1955*a*). Besides general intelligence, it measures fifteen other factors, some of which are well known from clinical or general psychology; e.g. cyclothymia *v.* schizothymia (Factor *A*), emotional integration *v.* neuroticism (Factor *C*), surgency *v.* desurgency (Factor *F*), dominance *v.* submission (Factor *E*), etc., while others are new, e.g. sensitive emotionality *v.* toughness (Factor *I*) and self-sufficiency (Factor Q_2). Each factor score is based on ten or thirteen items in the *A* Form and the same number in the *B* Form, and the reliabilities of the factor scores (not to be confused with that of the items, by re-test) average 0·69. The whole test, which is untimed, takes from three-quarters of an hour to two hours.

Profiles, as means and sigmas for each factor, are already known on the 16 P.F. for the student population, the general adult population (Cattell, Stice & Saunders, 1950), and about twenty rather varied occupations. In addition to comparing the researcher's profile with these, we wished to compare it more sharply with those of teachers and administrators in the same areas, as indicated above. The areas taken were three: Physical Science (represented by Physics); Biological Science (represented by Biology as such); and Psychology (which, as Roe's results (1953) suggest, may be taken as representative to some extent of the social sciences).

Committees were set up to choose from the roster of their professional society members, especially the starred scientists (Visher, 1947; Cattell & Brimhall, 1921), between 200 and 250 individuals in each of the three areas. Since we were more interested in the researcher than in the others, they were instructed to choose about twice as many researchers as teachers and administrators (typically 100 R, 50 T, and 50 A). By definition the subjects were to be eminent or 'successfully engaged' in their profession, and specifically noted, in the one group, for research contribution, in the second for teaching (including text-book writing), but with low or negligible research publication (less than one article in 3 years), and, in the third, for administration (heads of departments, national committee organizers, board members, etc.), again with negligible comparable research contribution. For uniformity in an initial study we took American scientists, rather than British and American mixed, until such time as the test standardization should become wider in Britain. The numbers chosen and responding are shown in Table 1.

Table 1. *Samples of leading faculty members studied*

Later reference no. of group		No. designated	No. agreeing to co-operate	No. satisfactorily completing questionnaire	Mean and sigma of age of completers
1	Teachers	62	29	29	44·7 (8·1)
2	Administrators	62	27	21	50·1 (9·1)
3	Researchers	93	55	46	48·7 (9·4)
	Subtotals	217	111	96	
	Physicists				
4	Teachers	50	28	25	43·0 (10·9)
5	Administrators	55	25	20	53·1 (8·3)
6	Researchers	121	53	46	51·4 (12·2)
	Subtotals	226	106	91	
	Psychologists				
7	Teachers	52	30	27	42·1 (9·0)
8	Administrators	48	33	28	48·0 (5·8)
9	Researchers	105	68	52	42·9 (8·1)
10	Selected researchers*	—	68	39*	43·2 (8·7)
	Subtotals	205	131	107	
	Grand total	648	348	294	

* This group of 39 was selected from the 52 by more exacting standards as to significance of research. It is interesting to see that in subsequent results it behaves 'like the research group (no. 9) only more so'. Not re-included in grand totals.

As usual we have no control on the selection by rejection, but it is at least satisfying that the percentage thus lost is surprisingly steady for different specialities and groups. We end, as we began, with about twice as many in the research group as in each of the other groups. The ages are also fairly uniform (the psychologists running somewhat

younger) and high enough for the men to have had their work stably assessed by time. The selection of respondents by special committees rather than by simply taking, for example, starred men from *American Men of Science* (Visher, 1947), was required by various considerations, but chiefly by the need for separate ranking of general scientific faculty within the subgroups of teachers, administrators, and researchers.

The analysis of results is made in terms of means and significances of difference of means (first in raw scores, then in standard scores) on each factor. Analysis of variance would have no particular point here, and indeed our later purposes call simply for a basis for applying pattern similarity coefficients, or *Q*-technique, or multiple-discriminant functions, according to the present evidences as to which factors are significant for selection.

III. Differences from the general adult population

The raw score means and sigmas for each of the ten groups on all factors are shown in Table 2 (*a*). In Table 2 (*b*), which immediately follows, these values are converted into ten-point standard scores (mean = 5·5; sigma = 2 points) according to table 5 in tabular supplement A, for the Sixteen Personality Factor Handbook (Cattell *et al.* 1950). These norms are for a general adult population averaging 38 years, made by combining various occupational groups in proportions indicated by the census (Cattell *et al.* 1953). No age correction has been made, because knowledge of trends in the personality factors is still not accurate, and our groups are only a few years older (4–15 years) than the standardization group. Actually, the effect of the age trends as at present assessed is never more than 0·2 raw score point per year. In terms of standard scores, allowance for such trends would reduce slightly (about 1 point for 10 years) the levels in *H* and Q_3, and raise at about the same rate or less, the calculated levels in *B*, *F*, *O*, and Q_4, relative to those shown in Table 2 (*b*). Such a correction would apply to Biologists, A(dministrators) and R(esearchers); Physicists, A and R, and Psychologists, A and R′.

It will be seen at once that this group of leading scientists departs markedly from the pattern of the general population, and in a very consistent fashion, despite differences of field or mode of activity. They are in general more intelligent, more schizothyme, of greater ego-strength and dominance, less surgent, more adventurous, more sensitive, have less free-floating anxiety, and are slightly more radical and more self-sufficient. On super-ego strength, which is represented in a dual way, not yet entirely clear, by Factors *G* and Q_3, the evidence is conflicting. The Factor *G*, which represents emotional adoption of standard moral goals, is lower; but Factor Q_3, sometimes called 'will control' and which represents strict, internalized and intellectualized standards, and exacting demands on the self, is decidedly higher.

The reader may examine, from the sigmas in Table 2 (*a*) and the Standardization Table (Cattell *et al.* 1950), the significance of the 160 possible differences, but for economy of presentation we shall group the data into teachers (biologists, physicists, and psychologists together), administrators (similarly grouped), and researchers (similarly grouped), thus reducing to forty-eight comparisons. These are shown, as raw score differences with associated *P* values, in Table 3.

Thus, without attention to differences *among* teachers, administrators, and researchers, which are examined in the next section, we can see that the deviations from the general population listed from Table 2 (*b*) proved to be uniformly significant for all three groups.

Table 2 (*a*). *Raw scores. Means and sigmas for ten groups on all factors*

	A	*B*	*C*	*E*	*F*	*G*	*H*	*I*	*L*	*M*	*N*	*O*	Q_1	Q_2	Q_3	Q_4
Group 1.	Biologists: T (29)															
Mean	11·79	20·41	34·03	23·03	18·90	23·10	27·76	18·83	17·24	19·24	24·14	12·21	25·55	25·76	27·48	13·93
Sigma	5·10	1·97	4·66	6·86	6·93	3·63	11·00	3·97	4·16	5·21	3·34	6·94	5·36	5·44	4·77	6·51
Group 2.	Biologists: A (21)															
Mean	13·81	21·57	33·71	24·52	22·76	23·95	31·43	20·62	17·00	19·95	25·10	14·10	24·19	24·81	27·33	16·38
Sigma	5·24	2·82	5·09	6·12	5·64	4·97	7·66	3·71	5·08	4·77	4·89	8·12	4·07	6·85	4·84	8·11
Group 3.	Biologists: R (46)															
Mean	10·02	21·43	31·96	22·41	18·35	23·91	27·54	20·87	16·50	21·24	24·30	13·26	25·87	28·22	27·48	14·52
Sigma	4·31	2·46	4·82	5·18	6·93	5·36	9·41	5·13	4·24	5·51	3·53	8·01	5·41	6·32	4·71	7·73
Group 4.	Physicists: T (25)															
Mean	10·88	21·00	34·84	20·16	20·72	22·68	25·20	18·96	17·32	20·20	23·92	14·20	24·12	24·60	27·40	15·96
Sigma	4·15	2·50·	4·65	5·69	8·67	6·00	10·31	3·75	3·65	5·64	3·95	6·04	5·09	5·72	5·68	7·54
Group 5.	Physicists: A (20)															
Mean	8·65	20·45	35·35	20·00	18·50	26·00	28·00	17·50	15·75	15·95	24·70	13·25	21·65	25·80	29·25	13·25
Sigma	3·67	2·76	5·23	6·00	5·23	4·60	9·62	3·93	3·51	4·30	3·29	8·22	4·42	4·41	4·15	5·87
Group 6.	Physicists: R (46)															
Mean	10·13	21·96	34·61	23·20	19·78	22·93	27·87	19·37	15·70	19·96	24·37	12·41	24·17	27·00	27·26	13·63
Sigma	3·99	2·47	4·14	5·78	6·96	6·26	10·04	3·96	4·62	7·25	4·10	7·62	4·27	4·10	5·21	7·17
Group 7.	Psychologists: T (27)															
Mean	16·19	21·93	33·48	25·37	22·63	20·19	32·52	20·37	17·44	23·37	23·63	13·93	27·41	24·30	25·44	17·41
Sigma	4·79	2·40	6·59	6·26	6·29	5·59	8·55	4·91	5·54	5·42	3·85	8·25	4·97	4·88	4·41	9·13
Group 8.	Psychologists: A (28)															
Mean	19·71	21·93	37·25	24·32	24·61	21·11	36·82	18·46	14·64	20·32	24·50	8·18	25·18	22·29	17·75	10·18
Sigma	5·14	2·00	4·12	5·17	6·64	5·60	7·60	4·73	4·08	4·41	2·91	7·12	4·57	5·46	4·90	6·03
Group 9:	Psychologists: R (52)															
Mean	13·33	21·83	32·60	27·27	24·21	20·37	32·23	19·04	18·46	23·06	24·00	13·46	26·98	26·04	24·33	14·92
Sigma	5·29	2·87	5·98	5·78	6·83	4·77	9·78	4·43	4·84	6·34	4·45	7·20	5·23	6·74	6·39	6·90
Group 10.	Psychologists: R′ (select) (39)															
Mean	13·26	21·67	32·38	26·72	23·21	20·33	30·92	18·54	18·05	22·44	23·95	13·26	26·95	26·51	24·26	14·92
Sigma	5·73	2·96	5·88	6·29	6·66	4·96	10·35	4·49	5·01	6·22	4·78	6·85	5·25	6·98	6·48	6·66

Table 2 (*b*). *Standard scores. Means for ten groups on all factors*

	A	*B*	*C*	*E*	*F*	*G*	*H*	*I*	*L*	*M*	*N*	*O*	Q_1	Q_2	Q_3	Q_4
Group 1. Biologists: T																
Mean	3·58	6·38	7·10	6·70	2·88	3·69	6·20	6·76	4·24	4·97	5·47	3·42	6·15	6·24	7·09	4·99
Group 2. Biologists: A																
Mean	4·44	6·73	7·03	7·28	4·08	4·02	6·93	7·43	4·14	5·23	5·91	3·94	5·79	6·02	7·05	5·66
Group 3. Biologists: R																
Mean	2·83	6·69	6·66	6·47	2·70	4·00	6·16	7·53	3·94	5·70	5·55	3·71	6·24	6·82	7·09	5·15
Group 4. Physicists: T																
Mean	3·19	6·56	7·27	5·60	3·44	3·53	5·70	6·81	4·27	5·32	5·37	3·97	5·77	5·97	7·07	5·54
Group 5. Physicists: A																
Mean	2·24	6·39	7·38	5·54	2·75	4·81	6·25	6·25	3·64	3·77	5·73	3·71	5·11	6·25	7·54	4·80
Group 6. Physicists: R																
Mean	2·87	6·85	7·23	6·77	3·15	3·63	6·23	6·96	3·62	5·23	5·58	3·48	5·79	6·54	7·03	4·91
Group 7. Psychologists: T																
Mean	5·45	6·84	6·99	7·60	4·04	2·57	7·15	7·34	4·32	6·47	5·24	3·89	6·65	5·90	6·57	5·94
Group 8. Psychologists: A																
Mean	6·95	6·84	7·79	7·20	4·66	2·93	8·00	6·62	3·20	5·36	5·64	2·32	6·05	5·43	7·16	3·96
Group 9. Psychologists: R																
Mean	4·24	6·81	6·80	8·33	4·53	2·64	7·09	6·84	4·72	6·36	5·41	3·76	6·53	6·31	6·29	5·26
Group 10. Psychologists: R′																
Mean	4·21	6·76	6·75	8·12	4·22	2·63	6·83	6·65	4·56	6·13	5·39	3·71	6·53	6·42	6·27	5·26

In intelligence, ego strength, dominance, desurgency, lack of group super-ego, adventurousness, sensitive emotionality, freedom from paranoid trends, freedom from free-floating anxiety, and higher 'internalized compulsive super-ego' (formerly labelled 'will control') the differences for all three groups are significant away beyond the 1% level. In schizothymia, radicalism, and self-sufficiency the uniformity is broken by the Administrator group's divergence falling to only the 1–10% significance level.

It is interesting to compare these deviations of the 'genus' *successful academic man*, as measured from the 'order' *general population*, also with their deviations from the 'family' *general university student*, as shown by undergraduate norms. Due to the large age difference that would here be involved, and the impossibility of accurately bridging it with present knowledge of personality factor life changes, no table has been set out. However, from table 3 in Supplement A of the 16 P.F. Handbook (university undergraduate students, men and women), and using the allowances for age trends over 25 years, given in table 6 therein, we can at least state indications. With the student profile thus projected forward 25 years the outstanding faculty now show no difference on ego strength (C), dominance (E), super-ego: group standards (G), sensitive emotionality (I), free anxiety (O), and internalized super ego (Q_3). They switch over on adventurousness (H) from high to somewhat low, on paranoid trend (L) from low to somewhat high, and on psychosomatic anxiety (Q_4) from slightly low to slightly high. They still deviate, almost as much as before, in the direction of being more intelligent (B) and more radical (Q_1). Finally, in schizothymia ($A-$) and self-sufficiency (Q_2) they deviate as they did from the general population, only more so!

The only uncorrected influence of unknown magnitude in the above is from the larger proportion of males (see Table 1) in the outstanding faculty than in the standardization groups for students and adult occupational composites. However, surprisingly few significant differences have been found between male and female populations, either in the form or the level of their general personality factors. The only observed differences significant at the 1% level (Cattell *et al.* 1950, 1953) are for women to be more cyclothyme ($A+$), less dominant ($E-$), and more sensitively emotional ($I+$). On this basis, if more balanced for sex, our outstanding academics might deviate less in schizothymia and dominance, and show some slight deviation from the student group in the same direction as from the general population in Factor I, toward sensitive emotionality. If entirely male norms were substituted for mixed norms in our control general population and student groups the deviation of academics on these three sex-modifiable factors would still be in the direction of greater schizothymia, dominance, and sensitive emotionality, regarding the general population, but dominance would cease to have significant deviation from male students.

These differences of pattern against different backgrounds may become confusing to the reader without time to study the various tables and corrections, so in conclusion we will concentrate on the contrast with the student type. Here it is interesting to observe that the deviations of these successful professional men are in agreement with those found by Wright (1954) to distinguish men of equal intelligence but higher scholastic achievement from those of lower achievement. His differences significant at $P=0{\cdot}01$ were: greater schizothymia ($A-$) and withdrawn schizothymia ($H-$); greater ego strength (C), absence of free-floating anxiety ($O-$) and of somatic anxiety (Q_4-). The three last do not distinguish our men from students, but do distinguish them from the general population. To

researchers (R), from general population

(Positive scores indicate *higher* than the general population in the usual direction of measurement of the factor, i.e. that of the name on the left of the bipolar title.)

	T		A		R	
	Diff.	*P*	Diff.	*A*	Diff.	*P*
A. Cyclothymia *v.* schizothymia	−3·32	<0·001	−1·59	0·03	−5·05	<0·001
B. Intelligence *v.* defective general ability	3·60	<0·001	3·89	<0·001	4·24	<0·001
C. Ego Strength *v.* general emotionality	7·60	<0·001	9·12	<0·001	6·53	<0·001
E. Dominance *v.* submissiveness	3·03	<0·001	3·23	<0·001	4·52	<0·001
F. Surgency *v.* desurgency	−6·60	<0·001	−5·20	<0·001	−6·38	<0·001
G. Super-ego, identification with group standards *v.* lack of super-ego	−5·80	<0·001	−4·41	<0·001	−5·48	<0·001
H. Adventurous cyclothymia *v.* withdrawn schizothymia	4·36	<0·001	8·42	<0·001	5·14	<0·001
I. Sensitive emotionality *v.* toughness	3·88	<0·001	3·34	<0·001	4·23	<0·001
L. Paranoid trend *v.* lack of paranoid trend	−3·07	<0·001	−4·72	<0·001	−3·45	<0·001
M. Bohemian unconcern	0·21	0·37	−1·76	<0·01	0·79	0·08
N. (Psychological title uncertain)	−0·30	0·25	0·54	0·13	0·02	0·48
O. Free-floating anxiety *v.* lack of anxiety	−6·40	<0·001	−8·35	<0·001	−6·74	<0·001
Q_1. Radicalism *v.* conservatism	2·63	<0·001	0·76	0·10	2·63	<0·001
Q_2. Self-sufficiency *v.* lack of self-sufficiency	2·31	<0·01	1·47	0·02	4·44	<0·001
Q_3. Super-ego, will control *v.* lack of will control	5·58	<0·001	6·86	<0·001	5·07	<0·001
Q_4. Psychosomatic anxiety	−0·08	0·46	−2·84	<0·01	−1·42	0·01

Table 4. *Means and sigmas for teachers, administrators, and researchers on all factors*

	A	*B*	*C*	*E*	*F*	*G*	*H*	*I*	*L*	*M*	*N*	*O*	Q_1	Q_2	Q_3	Q_4
Teachers (81)																
Mean	12·98	21·10	34·10	22·93	20·70	22·00	28·56	19·38	17·33	20·91	23·90	13·40	25·73	24·91	26·78	15·72
Sigma	5·20	2·35	5·34	6·58	7·39	5·22	10·35	4·25	4·47	5·64	3·67	7·12	5·26	5·32	4·99	7·82
Administrators (69)																
Mean	14·71	21·39	35·62	23·13	22·28	23·39	32·62	18·84	15·68	18·94	24·74	11·45	23·86	24·07	28·06	12·96
Sigma	6·61	2·54	4·93	5·98	6·40	5·47	8·95	4·33	4·31	4·83	3·67	8·12	4·56	5·78	4·67	7·09
Researchers (144)																
Mean	11·25	21·74	33·03	24·42	20·92	22·32	29·34	19·73	16·95	21·49	24·22	13·06	25·73	27·04	26·27	14·38
Sigma	4·83	2·61	5·17	5·97	7·31	5·64	9·92	4·57	4·71	6·49	4·04	7·56	5·10	5·91	5·68	7·23

Wright's picture our study adds that the scientific 'achievers' are higher in intelligence (*B*), radicalism (Q_1), self-sufficiency (Q_2), and perhaps sensitive emotionality (*I*) and paranoid trend (*L*). In personality they are thus not simply 'academic over-achievers', and indeed, considered in the light also of their profile deviations from the general population, which is the more basic comparison, they are distinguished, in addition to the above 10 factors, by high dominance (*E*), high internalized super-ego or will control (Q_3) and marked desurgency (*F*−).

It is to be hoped that these findings on well known factors will lead to fuller understanding of the psychological conditions of and selection pathways toward scientific eminence, as well as to additional insight and predictive power in regard to the psychological nature of the personality factors themselves.

IV. The differences of researchers from administrators and teachers

Having defined the genus of 'outstanding academic scientists', we can now discover within it the characters of the particular species—the researcher—in which we are primarily interested.

Table 4 sets out the personality factor means and sigmas for the three groups (from which the values both of Table 3 and Table 5 are derived).

The significance of differences of means among the Table 4 groups is shown in Table 5. The *P* values are for a two-tailed distribution, i.e. assuming no hypothesis has been made as to the direction of difference, though actually hypotheses in some cases *were* made (Cattell, 1955*b*) and the *P* values might be halved in those instances.

Table 5. *Significance of differences among teachers* (*T*), *administrators* (*A*), *and researchers* (*R*)

(Positive differences indicate higher mean scores in the category mentioned second in each comparison; measurement in the same direction as in Table 3.)

	T *v.* A		T *v.* R		A *v.* R	
	Diff.	*P*	Diff.	*P*	Diff.	*P*
A. Cyclothymia-schizothymia	1·73	0·08	−1·73	0·01	−3·46	0·001
B. Intelligence	0·29	0·47	0·64	0·07	0·35	0·36
C. Ego strength	1·52	0·07	−1·07	0·15	−2·59	0·001
E. Dominance	0·20	—*	1·49	0·09	1·29	0·15
F. Surgency	1·58	0·16	0·22	—	−1·36	0·17
G. Super-ego	1·39	0·11	0·32	—	−1·07	0·18
H. Adventurous cyclothymia	4·06	0·01	0·78	—	−3·28	0·02
I. Sensitive emotionality	−0·54	0·44	0·35	—	0·89	0·17
L. Paranoia	−1·65	0·02	−0·38	—	1·27	0·07
M. Bohemian unconcern	−1·97	0·02	0·58	0·46	2·55	0·001
N. (Psychological title uncertain)	0·84	0·23	0·32	—	−0·52	0·36
O. Free anxiety	−1·95	0·12	−0·34	—	1·51	0·19
Q_1. Radicalism	−1·87	0·02	0·00	—	1·87	0·01
Q_2. Self-sufficiency	−0·84	0·37	2·13	0·01	2·97	0·001
Q_3. Super-ego, will control	1·28	0·11	−0·51	0·48	−1·79	0·02
Q_4. Psychosomatic anxiety	−2·76	0·02	−1·34	0·21	1·42	0·17

* Any *P* value beyond 0·50 is represented by a —.

Suggestive differences are visible in the tendency of researchers to have higher intelligence (*B*) than teachers ($P = 0{\cdot}07$), to be more dominant (*E*) than either teachers ($P = 0{\cdot}09$) or administrators, and to be more paranoid (*L*) than administrators ($P = 0{\cdot}07$). Differences immediately acceptable at the $P = 0{\cdot}05$ to 0·01 level are: the tendency to more

withdrawn schizothymia ($H-$) and to lower compulsive super ego (Q_3) than administrators. Any difference is a contribution to definition of both parties, and some of the above are as much comments on the more outgoing qualities of administrators as on the more withdrawing qualities of researchers; but finally we shall stress the definition of the researcher in terms of the ways he differs simultaneously from administrators and teachers.

Thus, at or beyond the 1% level of certainty he is more schizothyme ($A-$) and more self-sufficient (Q_2) than teachers or administrators. At the same level he shows more emotional instability ($C-$) and Bohemian unconcern (M) than administrators, but in this case teachers are similar to the administrator block at a less clear P level. Also beyond the 1% level is the researcher's tendency to be more radical (Q_1) than the administrator.

Table 6. *Significance of differences among teachers, administrators and researchers for separate scientific areas*

Read as for table 5; italicized figures refer to 'Selected researchers' (R′) (see Table 1).

	T *v.* A		T *v.* R		A *v.* R		R *v.* R′	
	Diff.	*P*	Diff.	*P*	Diff.	*P*	Diff.	*P*
Biologists								
H. Adventurous cyclothymia	3·67	0·08	−0·22	0·46	−3·89	0·04		
I. Sensitive emotionality	1·79	0·05	2·04	0·03	0·25	0·41		
L. Paranoia	−0·24	0·43	−0·74	0·23	−0·50	0·35		
M. Bohemian unconcern	0·71	0·31	2·00	0·06	1·29	0·16		
Q_3. Super-ego, will control	−0·15	0·46	0·00	0·50	0·15	0·45		
Q_4. Psychosomatic anxiety	2·45	0·23	0·59	0·36	−1·86	0·19		
Physicists								
H. Adventurous cyclothymia	2·80	0·17	2·67	0·15	−0·13	0·48		
I. Sensitive emotionality	−1·46	0·10	0·41	0·33	1·87	0·04		
L. Paranoia	−1·57	0·07	1·62	0·05	−0·05	0·48		
M. Bohemian unconcern	−4·25	<0·01	−0·24	0·44	4·01	<0·01		
Q_3. Super-ego, will control	1·85	0·10	−0·14	0·46	−1·99	0·05		
Q_4. Psychosomatic anxiety	−2·71	0·09	−2·33	0·10	0·38	0·41		
Psychologists								
H. Adventurous cyclothymia	4·30	0·02	−0·29	0·44	−4·59	0·01		
			−1·00	*0·25*	*−5·90*	*<0·01*	*−1·31*	*0·27*
I. Sensitive emotionality	−1·91	0·07	−1·33	0·44	0·58	0·01		
			−1·83	*0·25*	*0·08*	*<0·01*	*−0·50*	*0·27*
L. Paranoia	−2·80	0·02	1·02	0·21	3·82	<0·001		
			0·61	*0·32*	*3·41*	*<0·01*	*−0·41*	*0·35*
M. Bohemian unconcern	−3·05	0·01	−0·31	0·41	2·74	0·01		
			−0·93	*0·26*	*2·12*	*0·05*	*−0·62*	*0·32*
Q_3. Super-ego, will control	2·31	0·03	−1·11	0·18	−3·42	<0·01		
			−1·18	*0·19*	*−3·49*	*0·01*	*−0·07*	*0·48*
Q_4. Psychosomatic anxiety	−7·23	0·01	−2·49	0·11	4·74	<0·01		
			−2·49	*0·11*	*4·74*	*<0·01*	*0·00*	*0·50*

Since some differences may be partly peculiar to certain scientific areas, and thus may be masked by arranging all areas together, about one-half of the total possible area breakdowns—those of sufficient interest to justify space—are presented in Table 6. (All have been examined and the full table is presented elsewhere (Cattell, 1955*b*)). In general we may note a tendency for differences to be more significant among psychologists, reflecting either greater specialization or greater skill of that selection committee (motivated by both scientific and psychological interests) in sorting out the criterion types. There is also a tendency for the more selected psychological research group, R′, to show more significant differences than the larger group, R.

The agreement of the differences for the subgroups in contributing to those 0·01 level differences for the total group cited from Table 5 is so uniform that only one of those five factors needs to be relisted here in order to inspect an inconsistency in the parts. This is *M*, Bohemian unconcern, where it will be seen that there is an inversion in psychology, teachers being slightly more 'Bohemian' (*M*+) than researchers. Factor *E*, not reproduced here, also shows an inversion of the usual greater dominance of researchers in the special case of biology, and these biological researchers are also significantly ($P=0{\cdot}01$) more desurgent (*F*−) than their colleagues, which suggests perhaps aesthetic rather than mastery interests in much biological research.

Turning to the differences that are of rather low significance in the composite picture and examining them in individual areas we note first that the higher 'paranoid trend' score is really peculiar to psychological researchers, in whom it reaches $P<0{\cdot}014$ and $<0{\cdot}001$. Possibly, in the more chaotic state of a new science, some tenacity of conceptualization is a necessary escape from despair! And perhaps those more interested in nature's laws than man's theories choose a more developed, structured science than psychology. The withdrawn schizothymia factor, *H*, is perceived in Table 6 to be no longer selective of researchers from teachers in the case of physics, but to be highly selective of psychological researchers, especially in the purer research group (R′). The effect is partly due to psychologist *teachers* being more adventurous cyclothymic (*H*+) than most. Presumably teaching in an area in which research has yet provided so little of an exact nature to talk about attracts a more sociable, adventurous type, perhaps more interested in the interactive process of teaching than in the subject-matter.

Factor I is included in Table 6 because it shows in each group a uniform tendency toward significance which is nevertheless missed in the composite picture. Researchers, relative in one case to administrators and another to teachers, tend to be more sensitively emotional (*I*+). Only the psychologists upset this. The trend is most clearly shown in the biologists, again suggesting aesthetic-emotional involvements in the motivation of biological research.

Factor *M* (Bohemian unconcern) is set out because the breakdown shows that the researchers' deviation is largely due to administrators being distinctly low in it. The latter show marked 'group concernedness' and indeed in physics and psychology the teachers are higher on unconcern than the researchers. Factor Q_3, internalized compulsive superego or will control is similar, in that the difference is principally due to the administrators showing the high regard for exactness, high standards of behaviour and rather obsessional trends which form the essence of the factor, though teachers are also higher than researchers. The same form of relation holds for Q_4, the absence of somatic anxiety being characteristic of administrators rather than its presence characteristic of researchers.

V. Summary and discussion

1. The personality profile on the 16 P.F. Test has been compared for larger groups of scientists (96 biologists, 91 physicists, and 107 psychologists) than in any previous investigation of the personality of the researcher, and several differences significant at the $<0{\cdot}01$ or $<0{\cdot}001$ levels have been discovered: (*a*) between scientists and: (i) the general population, (ii) the student population; and (*b*) between researchers, teachers and administrators, all in the field of science.

2. The outstanding professional, academic scientists as a whole differ from:

(*a*) *The general population*, at beyond the 1% level, in the personality factors called general intelligence (*B*), ego strength or stability (*C*), dominance (*E*), desurgency (*F*−), lack of group super-ego standards (*G*−), adventurousness (*H*), sensitive emotionality (*I*), lack of paranoid trends (*L*−), lack of free-floating anxiety (*O*−), and compulsive super-ego (or will control) (Q_3). Except for administrators they are at the same significantly higher level in radicalism (Q_1) and self-sufficiency (Q_2). Administrators alone differ at the 1% level in showing lower somatic anxiety (Q_4−).

(*b*) The university *undergraduate population*, approximately age corrected, by being decidedly more schizothyme (*A*−) and self-sufficient (Q_2), more intelligent (*B*), desurgent (*F*−), withdrawn schizothyme (*H*−), radical (Q_1) and, probably, more paranoid (*L*) and of higher somatic anxiety (Q_4).

3. Researchers, relative to teachers or administrators, are, at the 1% level of certainty, more schizothyme (*A*−), self-sufficient (Q_2), emotionally unstable (*C*−), bohemianly unconcerned (*M*), and radical (Q_1). They are also significantly, but less uniformly, more dominant (*E*), paranoid (*L*), withdrawn schizothyme (*H*−) and lower on compulsive super-ego (will control) (Q_3). In the last and in radicalism they differ more from administrators than from teachers. There are some interesting differences indicated among the sciences, researchers in biology being, among biologists, less dominant and more desurgent; but these inter-area differences are more fully discussed elsewhere (Cattell & Drevdahl, 1955).

These results may be compared with Drevdahl's study (1954) of 'creative' *v.* 'non-creative' students in science and in the arts, which used the same personality factors and the same measuring instrument. The creative were significantly more schizothyme (*A*−), self-sufficient (Q_2), withdrawn schizothyme (*H*−), sophisticated (*N*+), desurgent (*F*−), and radical (Q_1). There is thus important agreement, while there are also some special characteristics of creative persons at the student level which do not particularly show up in those called to research (and vice versa).

Certain hypotheses had been reached from reading biographies of one hundred eminent British, American and other researchers prior to these experimental determinations (Cattell, 1955*b*). These personal records suggested that researchers would be more schizothyme, intelligent, dominant, paranoid, self-sufficient, and of greater super-ego development than the generality of professions of the same educational level. These characteristics of past researchers seem well matched by our findings on living researchers, except for super-ego strength, where the difference tends to be, if anything, reversed.

Brevity of space compels deferment of discussion of the psychological and social operation of these personality factors to another place (Cattell, 1955*b*). But it is easy to see that the schizothymic preoccupation with things and ideas, rather than people; the self-sufficiency which favours creativity and independence of mind; the dominance which gives satisfaction in mastery of nature for its own sake; and the emotional instability which permits radical re-structuring and creativity, would all be vital to the best kind of basic research performance—though perhaps unpleasant in an administrator and inapt in a business man. The remaining factors of Bohemian unconcern and radicalism may perhaps best be regarded as condition and consequence of continued adjustment to the researcher's comparative earnings!

It is suggested that until further research, especially with the improved personality

factor measures now becoming available (Cattell, 1954, 1955*a*), is prosecuted on this problem any vocational selection in progress be based on the standard score profile derived from row 5 of Table 4, and which is set out in standard form in Table 7. Naturally it should be used with due regard to its tentative character and in conjunction with whatever other evidence is available for selecting researchers from examination-passers at the university level.

Table 7. *Profile of the existing typical researcher (in general population standard scores)*

Factor		
A	Cyclothymia *v.* schizothymia	3·34
B	Intelligence	8·65
C	Stability	6·89
E	Dominance	7·24
F	Surgency	3·50
G	Super-ego, group standards	3·40
H	Adventurous cyclothymia	6·44
I	Sensitive emotionality	7·1
L	Paranoia	6·92
M	Bohemianism	5·78
N	—	5·50
O	Free-floating anxiety	3·66
Q_1	Radicalism	6·20
Q_2	Self-sufficiency	6·52
Q_3	Super-ego, will control	6·78
Q_4	Psychosomatic anxiety	5·11

As in other I.P.A.T. tests the 16 P.F. Standard Score is arranged in *Stens* not Stanines, i.e. each point is ½ a sigma and the population mean is 5·5, so that 1 and 10 are equidistant from the mean and 5·0 to 5·99 is essentially an average score.

This personality profile reminds us, after the above comparisons of the genus 'scientist' with the average, and the species 'researcher' with other specialists, of the essential divergence of the species 'researcher' from the average man. For example, though on the unstable side ($C-$) relative to other professionals in science he is yet decidedly above general, average stability. And though no more desurgent than other specialists he is yet emphatically below the general adult level of surgency.

Now the use of the pattern similarity coefficient, r_p (Cattell, 1949; Cronbach & Gleser, 1953) to determine the congruence of various propositi to the above profiles, when selecting promising men for research openings, involves certain assumptions which should be watched. First it gives equal weight to the various factors and secondly it assumes that the values for the existing researcher are the best values. While the first is the best guess with our present knowledge, and the second must be accepted unless we can claim to be wiser than society's laws of supply and demand,[1] yet we can hope to find out a little more. Just as the first step in vocational selection research is to define the type profile, so the second is to determine the linear or curvilinear relation of each factor to a criterion of success in a population *within* the occupation.

A beginning has fortunately been made by Van Zelst & Kerr (1954) with this second step, with results consonant with our own; but it needs tackling on an adequate scale and with radical attention to the criterion problem, perhaps along lines indicated elsewhere (Cattell, 1955*b*).

[1] As a psychologist, one may clearly realize that Factor B, intelligence, is likely to produce indefinite improvement of performance with higher values, but it is necessary also to be a sociologist and ask whether it is correct to advise a person to enter an occupation when his I.Q. is higher than that of persons at present normally attracted to, and perhaps more vitally needed in some other occupation.

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SUBJECTIVE JUDGEMENTS—SOME EXPERIMENTS EMPLOYING EXPERIENCED AND INEXPERIENCED OBSERVERS[1]

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I. Introduction

Design of equipment for use by human beings needs to be closely related to the detailed demands of the users. The human being himself can be the only satisfactory judge of the efficiency of the design. This fact has never been completely ignored, but there has been a tendency to direct most effort to the perfection of the physical measurement of design efficiency and to disregard the fact that to be of any real value measurement must be related to the judgement of the human being who is to use the equipment.

Recently there has been a healthy sign that this mechanistic attitude is giving way to other methods. Designs of equipment are tested systematically by people and not by physical measures. Undoubtedly one of the main causes of this change in outlook has been the introduction of simple statistical methods which enable subjective experiments to be designed soundly, and their results interpreted by a strict comparative statistical procedure, rather than by inspection and induction alone.

With these refinements in the technique of subjective judgement experiments has come realization that, if human beings are to be used as meters, they must be properly calibrated. The techniques used for calibration of human meters have revealed that some human meters drift in their calibrations with time. The period over which these drifts take place is often very long, perhaps months, and so quantitative evidence is hard to come by.

During the course of some lengthy studies on glare-discomfort, quantitative evidence of the behaviour of human meters has been collected which has been thought worth recording, particularly as this evidence seems to confirm well-established opinion. Quantitative evidence is still lacking for a satisfactory statistical treatment, but it has been considered worth while to put forward what is available at this stage because many workers in the field of subjective judgements appear to be unaware of these tendencies on the part of their subjects. These tendencies may influence their results and so lead to significant errors.

II. The performance of experienced and inexperienced subjects

The performance of subjects making mental assessments of their environment seems to be related to the experience of the subject of the particular phenomenon under study. It is unfortunately not too easy to define exactly what is meant by experienced and inex-

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perienced subjects. For the purposes of the discussion an experienced subject is considered to be one who is not only familiar with the technique of making mental assessments under experimental conditions, but who is also familiar with the nature of the problem being investigated. For example, an experienced subject in an experiment for the judgement of picture quality on a television screen, would be one who is not only familiar with the technique of judging the quality of photographs, that is to say, tone range, contrast, definition and so on, but who is also familiar with the television problem itself, who has made judgements of a number of different systems and is aware of the nature and importance of the variables being studied. An inexperienced observer is one who has not these advantages.

The main experiments referred to in this paper are concerned with the judgement of glare discomfort in buildings. The background to the work, however, extends back many years and includes detailed subjective judgement experiments on the assessment of the quality and luminosity of photographs for true representation, the judgement of ease of reading under a wide range of illumination and contrast conditions, the assessment of glare discomfort in lighted streets, various wartime visibility problems such as the assessment of the visibility of radar signals under a wide variation of display types, and so on.

Three conclusions emerged very early in the attempts at systematic subjective judgements.

(1) Subjects with experience of the phenomena under investigation gave observations with a greater repetition accuracy than those of inexperienced subjects.

(2) The mean of observations from subjects selected at random from the population was sometimes, but not always, significantly different from the mean of observations from skilled observers.

(3) The diversity of opinion between inexperienced observers was often greater than that between experienced observers.

A considerable amount of data led to the belief that, if reasonably speedy progress were to be made in an experiment involving subjective assessments, it would be preferable every time to build up a small panel of five or six skilled observers to make the judgements, rather than to hope for anything useful to be achieved in a reasonable time with observers selected at random from the population.

During the war when speed was essential, this technique of relying on a small team of skilled observers was fully justified. Care had to be taken, however, to use the team only for those types of experiments where it could be reasonably certain that the mean values of the observations given by the small team would be sensibly the same as those given by the population who were going to use the particular piece of equipment under test.

There was evidence right from the start that in certain types of judgement the skilled team gave observations different from the general population, although this was by no means always the case. The distinction between the observations of inexperienced and experienced subjects depended on the nature of the phenomena under investigation.

For example, in the estimation of discomfort, the experienced observer would recognize consistently the existence of a sensation of discomfort under conditions which the inexperienced observer considered to be satisfactory. On the other hand, in the estimation of visibility the experienced observer would be able to recognize detailed characteristics of a signal consistently under conditions which would barely permit the inexperienced observer to observe its presence. With both these types of experiment the distinction

between the experienced and the inexperienced observers shows itself up by differences in their mean assessments as well as in the variability of their observations.

In other experiments however, for example, the grading of contrast scales in steps, no difference between the mean of the observations given by the experienced and the inexperienced observers shows itself. The distinction is apparent only in the much greater variance of the inexperienced observer's judgements.

III. Experimental studies of the effect of experience

An investigation of the effect of various physical factors on the sensation of glare discomfort in the lighting of buildings afforded an opportunity of studying the effect of experience on the sensitivity to discomfort. This study made use of the Multiple Criterion technique which includes a series of 'calibration' observations over a long period of time. The technique also demands the freedom on the part of the subject to change without restraint the physical conditions governing the situation in which he finds himself. This enables him to make settings corresponding to a predetermined series of criteria. Details of the technique have been described elsewhere (Hopkinson, 1950), but a brief recapitulation may be useful.

The essence of the Multiple Criterion technique is to present the subject with a stimulus situation in which he has control of one of the significant physical variables whose change influences the magnitude of the sensation which is being studied. No effort is made, however, to ask the subject to put any sort of numerical scale on the magnitude of the sensation. He is at first given the definitions of the series of related criteria of the sensation covering, if possible, the useful gamut of the sensation in not more than four distinct steps. He is then asked to adjust the magnitude of the physical variable to correspond, in turn, with each of this limited number of defined related criteria corresponding to the subjective response. In this way it is found that each criterion acts as a check upon judgements made in terms of the others. It gives the subject greater confidence in making his judgement. Differences between subjects in the setting of the physical variables for the same criterion of sensation should also be less attributable to differences in the interpretation of the description of the sensation.

A further essential feature of the technique is to have a 'control experiment' extending over the whole period of the investigation. This is effectively a calibration from day to day of the human being as a meter. The subject is presented with a series of standard situations in each of which everything is constant except one physical variable. He is then asked to adjust this particular physical variable to correspond in turn to a predefined series of criteria of sensation. This judgement is repeated with each of the situations. The same situations are presented to him day after day, and settings of the same control experiment permit an analysis to be made of the scatter of the subject's observations under standard conditions, so that confidence limits can be derived for other observations under similar conditions. It also permits any effect of the influence of experience to be detected by a progressive drift in the observations. It is therefore chiefly with the observations obtained under the 'controlled experiment conditions' that this discussion deals.

Fig. 1 shows a typical set of observations of such a 'controlled' experiment. The assessment here is of the surround brightness with which a given glare source gives 'just uncomfortable' glare discomfort (Petherbridge & Hopkinson, 1950). The surround brightness

is the physical variable under the observer's control, and he adjusts this to balance out the glare discomfort from the glare sources, which are fixed in size, position and brightness while the judgement is made. This observer is 'experienced' in the sense that he is a skilled photometrician and is familiar with the glare problem and with the variables that control it.

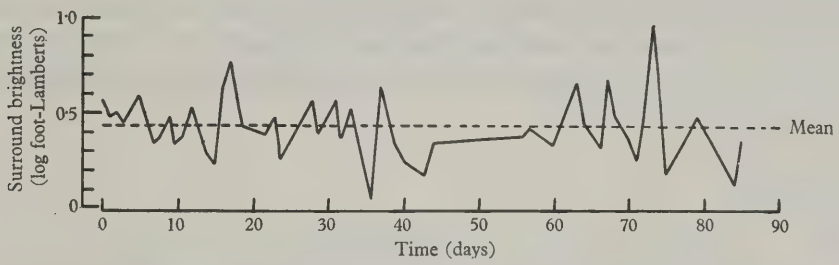

Fig. 1. Set of 'Control Experiment' observations showing an *experienced* observer's day-to-day assessments of the surround brightness with which given glare sources give 'just uncomfortable' glare. (Source brightness, 1000 ft.L.; observer P.P.)

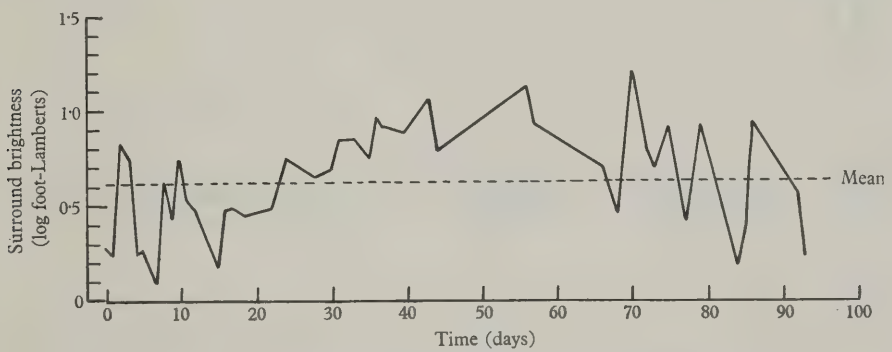

Fig. 2. Set of 'Control Experiment' observations given by an *inexperienced* observer. (Source brightness, 1000 ft.L.; 'just uncomfortable' glare; observer J.L.)

Throughout a period of nearly 90 days (except for a break of a fortnight in the middle of the series) observations made almost daily show no progressive trend. There are occasional observations which differ widely from the mean but, apart from these, the variance is quite small when it is remembered that the only guide which the observer has in making these observations is the degree of glare discomfort which he experiences. He has no means of recognizing any feature of the situation, and so cannot make his settings by some form of memory match.

These and similar data suggest fairly strongly that experienced observers who have formed a criterion of a sensation under controlled conditions can hold that criterion over a very long period. Considerable random variations will occur but the mean of a sufficiently large number of observations will remain stable.

A different situation appears to hold for inexperienced observers. Fig. 2 (observer J.L.) shows a set of observations taken by an observer who at the time had no experience of brightness judgements and of the glare problem. It can be seen that there has been a steady increase in the sensitivity to glare as the observer gains experience of the control experiment. (Other experiments in which the subject acted as an observer ran

concurrently with the 'control experiment', thus adding to his skill and experience from day to day.)

Similar trends are shown by other observers, as shown in Fig. 3.

The results shown in Figs. 2 and 3 were obtained independently of any direct attempt to study the effect of sensitization with onset of experience. Evidence of this kind, together with a great deal of qualitative evidence from previous experiments, and from anecdotal evidence from other work, led to a direct attempt to see if similar tendencies could be shown starting with observers specially selected to have no knowledge at all of the phenomena under investigation.

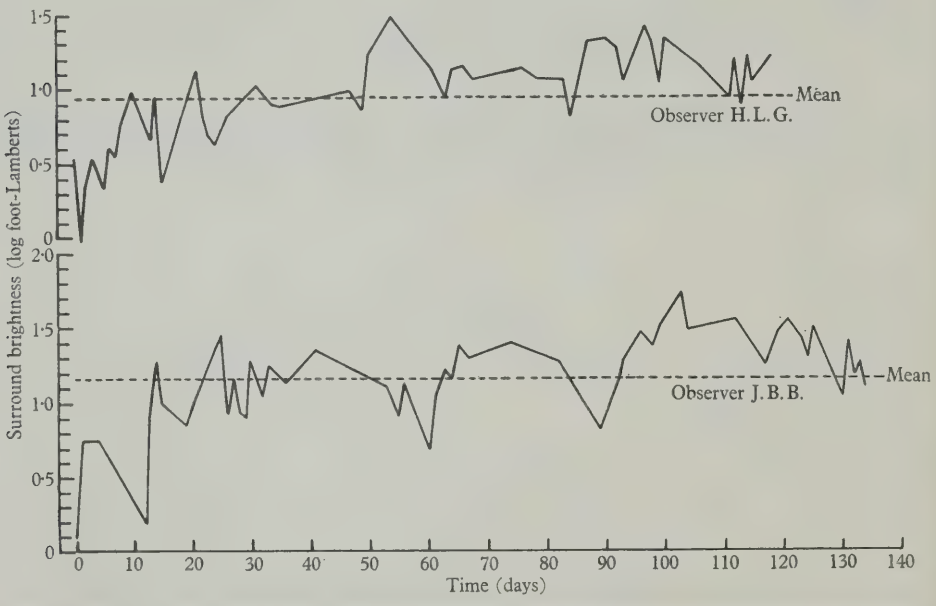

Fig. 3. Sets of 'control experiment' observations given by two further inexperienced observers when subjected to 'just intolerable' glare (source brightness, 3000 ft.L.).

Accordingly two observers were selected for this purpose. These observers were unfamiliar with lighting technology and had never been subjected to any questioning about glare or discomfort in lighting. They were, however, intelligent in their general approach to perceptual problems. Preliminary assessments were made which indicated that they were less sensitive to glare discomfort than the average experienced observer. Each observer subjected himself to the conditions of the control experiment exactly similar to those shown in Figs. 1–3. They were asked to make settings of the environment to give each of the series of degrees of glare discomfort under the controlled conditions, and these assessments were repeated at approximately daily intervals for several weeks. Observations by an experienced observer were made over a similar (but not concurrent) period under the same conditions. The results of this experiment have been reported elsewhere (Hopkinson, 1952) and are reproduced here (Fig. 4). An increase in glare sensitivity similar to that shown by the observer of Fig. 2 is shown here. Whereas the experienced subject has maintained his criterion over the period, the inexperienced subject has

gradually crept up in sensitivity (i.e. increased his setting of environmental brightness to balance out glare from the given glare sources) until his settings approached those of the experienced observer. After about 17 days his settings appear to have settled down to a fixed value. The other observer who performed the experiment showed the same trends. Unfortunately, the test could not be continued beyond the point shown in the illustration. Similar curves to those of Fig. 4 were obtained for other settings of the physical environment and for other criteria of glare discomfort. In all, twelve curves of this kind were derived for each observer, all showing the same tendencies.

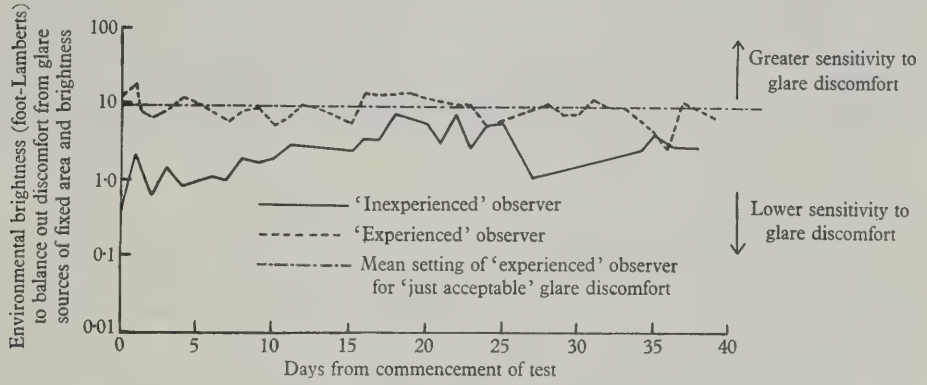

Fig. 4. Glare-discomfort assessments of an 'inexperienced' and an 'experienced' observer under identical conditions over periods of about 40 days for 'just acceptable' criterion, showing increasing sensitivity of 'inexperienced' observer to glare-discomfort over the period.

These observations give quantitative confirmation of the qualitative evidence referred to earlier in the paper. In the specific case of glare discomfort it is well known, from evidence accumulated from a long series of field studies of lighting, that a subject may be unconscious of glare discomfort from a bare lamp over his desk until his attention is directed to it by an investigator. He may then become aware of glare discomfort not only in his working conditions but in other environments as well. He has, in fact, become sensitized to his environment.

IV. Stability of the observer

The question remains whether the condition of the experienced observer is one of greater stability than that of the inexperienced observer. Insufficient evidence has been collected to lead to any firm conclusions. All the qualitative evidence does indeed point clearly towards the conclusion that the experienced observer retains his criterion over long periods of time, but the statistical evidence to support this opinion could with advantage be more conclusive. Fig. 5 which represents observations taken under control conditions by three observers over the period from September 1948 to January 1952, is apposite to the discussion. Two of these observers (P.P. and R.G.H.) come into the category of experienced subjects. The third (J.L.) was inexperienced when the observations first started, but subsequently made a great many readings. The observations, which are shown plotted, are again of the environmental brightness necessary to balance out glare discomfort from glare sources in the field of view to yield each of four criteria of glare sensation.

The observations made by the two experienced observers could be taken to support the claim that the experienced observer maintains his criterion.

There is little change in sensitivity to glare discomfort over the very long period under investigation, with some very long breaks between series of observations.

It is indeed remarkable that observers can maintain a criterion of a supposedly vague sensation such as 'discomfort' over such a very long period. Such variations as there are, are less than the differences between one criterion and the next, i.e. between 'just uncomfortable' and 'just acceptable'.

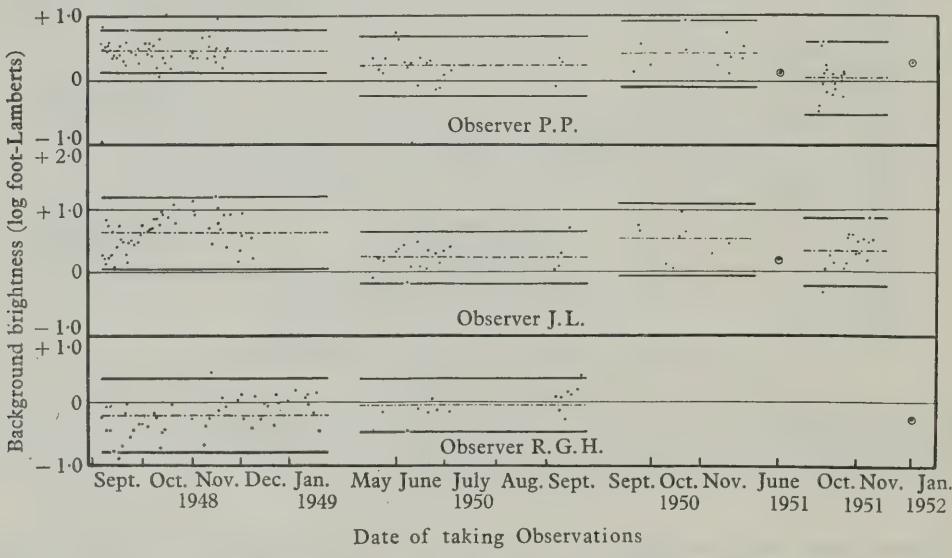

Fig. 5. Long-term glare sensitivities of three observers engaged in discomfort glare control experiments. Each dot represents an observer's setting of background brightness to give 'just uncomfortable' glare when the brightness of the sources is set at 1000 ft.L. The mean value and 95% confidence limits of each group of observations is shown. Single observations represented by '⊙'.

Fig. 5 shows mean values for each batch of observations, and the variance limits (in this case twice the standard deviation) are also shown for each observer's settings. The mean values and variance limits of the same observations have been analysed, and in addition the mean values and variance limits for the three other criteria of glare discomfort (just intolerable, just acceptable and just imperceptible glare discomfort). Similar sets of observations exist for other levels of source brightness.

On the whole, the observations confirm that the condition of the experienced observer is one of fair stability and therefore that such an observer can be used over an extended period of time as a meter of his surroundings. The experienced observers have held their criteria of glare discomfort over a period of three years with little change. If anything there is a tendency to a reduction in glare sensitivity, particularly by observer P.P., but the true significance of this change could only be assessed if observations were available for a much longer period of time.

The third observer, J.L., shows a wider variation in his settings. After his initial 'sensitizing period', he followed by a reduction in glare sensitivity rather in the nature of a slow but steady decrease towards, but not reaching, the lower sensitivity with which he

originally started the series of experiments in his inexperienced condition. This observer is more erratic in his day-to-day observations and so this decline in sensitivity has no greater statistical significance than the less marked changes in sensitivity of the first observer.

The rest of the data (for other observers, and other conditions) does not add materially to the picture presented above.

(1) The evidence is definitely in favour of the retention by the already experienced observer of his criterion.

(2) It is also definitely in favour of the existence of a 'sensitization period' for the inexperienced observer.

(3) The subsequent behaviour of the inexperienced observer after the sensitization period is over (he now having become experienced) is not established. Qualitative evidence is in favour of his ability to retain the criterion established at the end of his probationary period, but the quantitative evidence is inconclusive.

V. The selected team and the general population

It is hoped to continue the observations for a further period in order to establish whether or not subjective studies should, for preference, be conducted with the aid of a small team of experienced observers, each making a large number of measurements, or whether the preferable technique is to obtain a large number of measurements from single observations from a large number of observers selected at random from the population. From the practical point of view there can be no doubt about the answer. It is much more practicable to work with a small team of experienced observers, who in general make their observations quickly and confidently, and who are available in the laboratory when they are wanted, than it is to attempt to collect a very large number of observers and to make sure that they are selected from the correct cross section of the population. In fact it is almost certain that much subjective study work would have to be abandoned if the latter course were indicated.

The subjective studies in lighting at the Building Research Station are conducted with a small team of selected experienced observers in order to establish the relations between the relevant physical variables. These relations are then tested at one or two significant points by obtaining a large number of observations at these points from a cross-section of the relevant population. This is the only practical course open under present conditions.

The results of such comparisons between the observations of the team and the observations of the population are consistently in the direction of the greater sensitivity to discomfort of the team as compared with the population. The question then arises, how is the information obtained from the team to be used for the specification of conditions to be used by the population as a whole?

The problem at the moment is being solved in the following way. It is possible to prepare, in the form shown in Fig. 6, some of the extensive observations which have been collected from a selection of the general population. This relates the physical variable responsible for the particular sensation of visual discomfort to the proportion of the population who will suffer discomfort at any given level of that variable. The mean value of the physical variable given by the team to correspond to the same criterion of discomfort sensation is also shown in Fig. 6 for the relevant conditions. It is seen that in this case

approximately 85 % of the population would experience a lower level of discomfort than the team. Comparable probability curves have been drawn for other situations. It has been found that the mean values of the physical variable which the team observations yield correspond fairly well to a value of about 85 %.

It has therefore been decided for the moment that in recommendations to bodies concerned with the specification or the recommendation of physical levels concerned with visual comfort (for example levels of illumination for visual tasks, levels of brightness for lighting units, etc.) the mean values of the physical variable as given by the team of observers will be offered. There is then evidence that approximately four-fifths of the

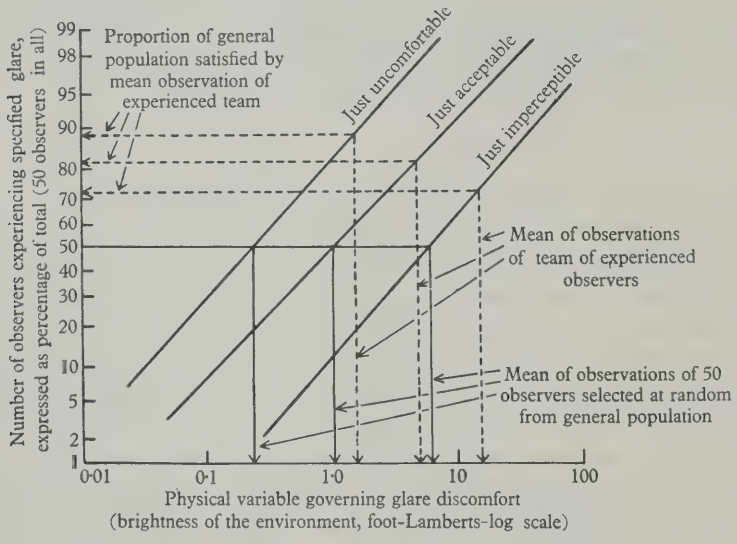

Fig. 6. Relation between mean settings of team of experienced observers and the observations of the general population on glare discomfort.

population would also be satisfied by these conditions. This seems a preferable course to taking the mean value given by the population as a whole, which of course would lead to half only of the observers of the population being properly accommodated. When further information is available on the nature and reasons for these differences between the judgements of experienced and inexperienced observers it may be possible to come to conclusions based on less arbitrary decisions.

VI. Discussion

The nature of the study demands a very long time to complete the work. The particular study, that of the estimation of glare discomfort in lighting, is also not one which lends itself readily to a high degree of repetition accuracy. However, it seems reasonable to draw the following conclusions from the observations which are so far available:

(1) In general, inexperienced subjects are less demanding in their requirements to avoid visual discomfort, indicating that they are therefore less sensitive to glare discomfort than experienced subjects.

(2) Some inexperienced subjects when put through an identical series of tests over a long period become sensitized, and their requirements then agree with those of experienced subjects.

(3) Some subjects appear to reach a steady state beyond which no further experience of the situation creates further demands. This is in accord with qualitative findings. There may in fact be a subsequent decline from the state of maximum sensitization. The evidence is clearer for the steady state than for the subsequent decline in sensitivity, but observations over a longer period are needed to establish statistical significance.

Beyond these conclusions it would be dangerous to go at present. The findings reported in the paper will probably confirm in their opinions those workers who already prefer to work with a small team of experienced observers.

There is another factor in the problem which must be brought into the discussion. Most workers in this field will have recognized the effect of personality in subjective judgement. A 'good observer' is co-operative and intelligent, is quick and keen to appreciate the points of technique of the experiment, and so learns to make his judgements readily and rapidly. A good observer also has a social conscience, and gives of his best, without in any way needing to have any knowledge of the trend of his results. It is indeed necessary that the observer should have no preconceived theories about the experiment on which he is engaged. Otherwise what has happily been called his 'coefficient of involuntary mendacity' may bias his judgements for or against his theory. The observer should be indifferent about the *result* of the experiment, but conscientious for the *success* of the experiment. The observer who makes no attempt to master the experiment, and who takes no care to relate his settings to his sensations, in fact who 'couldn't care less', is a nuisance, and his influence can only be eliminated by personal selection of the small team. The timid observer who is over-anxious to give a 'right' judgement is equally a nuisance for a different reason. Ideally such personal factors should be eliminated by random selection of observers, but with the small team technique this is of course not possible.

The practical implications of the conclusions are considerable. They raise economic factors; the experienced observer will demand higher quality equipment (for example for the reproduction of music or of pictures) for the same criterion of excellence as will the 'man in the street'. If the designer decides to plan only for the 'man in the street' he is able to produce a cheaper piece of equipment than if he designed for the experienced observer. This economic factor sometimes works in reverse. An experienced observer can often work usefully with inferior equipment; the novice may be incapable of detecting on a warning display signals which are readily apparent to the experienced man.

In other cases, design for the experienced observer may demand a change in design rather than a more expensive design. In the design of the lighting of buildings the elimination of glare discomfort must be more rigorous if the experienced observer is to be satisfied than if the judgement is by the average man. But here it is a matter of the redistribution of light to illuminate the surroundings as well as the work, to reduce the brightness of light sources, rather than the provision of more expensive lighting equipment.

Finally there are the implications of the conclusions on the methodology of subjective judgements. It is clear that any single judgement of a situation is subject to considerable variance. Even experienced observers deviate at times from their main judgements. What seems of greater importance, however, is the significant differences in the judgement of a situation by experienced and by inexperienced observers. The awareness of discomfort

is associated with experience of a situation, but can it be established that no harm comes to the inexperienced subject who is happily unaware of discomfort which is readily appreciable by the experienced man? Much more work would be necessary to establish the true state of affairs.

The existence of an 'experience factor' is of particular importance in view of the increasing use of the questionnaire techniques in social methodology. These are usually based on the single assessments of a large number of inexperienced observers. The error due to the variance of the judgements is eliminated in the statistical procedure of analysis, but the 'experience factor' remains. Can it be confidently accepted that it can be ignored?

VII. Summary

Experimental evidence has been obtained which shows that inexperienced subjects are less susceptible to glare discomfort than those who have had the opportunity to make a long series of judgements. A steady increase in sensitivity has been recorded. On the other hand, experienced subjects retain their enhanced sensitivity at a constant level over a period of years. Such experienced subjects are therefore valuable as human meters of their environment.

The comparison which has been made between judgements of such experienced subjects and those of inexperienced subjects shows that the average judgements of experienced subjects will satisfy about 85% of the inexperienced subjects.

Although all these judgements refer to a study of glare discomfort, it is believed that a similar relation exists in many other situations where judgement of an attribute allied with discomfort is involved.

The work described in this paper forms part of a research programme of the Building Research Board of the Department of Scientific and Industrial Research and the paper is published by permission of the Director of Building Research.

The data recorded in the paper were all taken incidental to an investigation into the physical variables which govern the degree of discomfort in lighting. The author gratefully acknowledges his indebtedness to his colleague Mr P. Petherbridge who conducted the experimental work, and to the observers who submitted themselves to the experiments.

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CHANGES IN TIME PERCEPTION INDUCED BY AN ANAESTHETIC DRUG

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I. *Introduction* (pp. 273–274). II. *Methods and Procedure* (pp. 274–275). III. *Results* (pp. 275–277). IV. *Discussion* (pp. 277–278). V. *Summary* (p. 278). *References* (pp. 278–279).

I. Introduction

Various changes in the perception of time have been reported following administration of drugs such as hashish (Pick, 1919; Bromberg, 1934), mescaline (Fröbes, 1923; Favilli, 1937; McKellar, 1954) and quinine (Sterzinger, 1938). Most of the data published have been observational and qualitative and there seems to have been little systematic follow-up.

During experiments with the anaesthetic gas nitrous oxide carried out by the present writer, some subjects spontaneously commented that time passed extremely slowly, e.g. 'minutes seemed like hours', while others said that time seemed to 'fly'; and a few described the effects as complete loss of sense of time. The purpose of the investigation to be reported was to explore whether such phenomena could be measured quantitatively, i.e. whether there was a relation between subjectively experienced changes in the rate of passage of time and estimates of the duration of time intervals made in accordance with a standardized procedure.

Some previous investigators have suggested that this might be so. Hoagland (1935) has sought to account for the impression of a patient suffering from influenza with fever that 'time seemed to pass very slowly' by assuming that the increased body temperature had made the patient's internal chemical timing mechanism run faster. This, he argued, 'should make time, as judged by objective clocks. . . appear to go by more slowly, since more psychophysiological "time" would pass in a given constant unit (a minute, let us say), of our conventional constant time scale standard'. In order to check, Hoagland asked the patient to estimate the duration of 60 seconds by counting. She completed the counts in considerably less than a minute, especially when the fever was relatively high. In a recent review Woodrow (1951) has referred to this finding as indicating that 'what was actually a minute by physical time seemed longer than a minute to the subject—an interpretation in agreement with the subject's report that time seemed to pass very slowly'. Case & Haldane (1941) have reported that during inhalation of air with carbon dioxide at high pressure time seemed to some of their subjects to be passing more quickly than usual, and in support they cite the finding that one of them took 60 seconds to tap out what he believed to be 30 seconds, while under normal conditions he took only 28. Assumptions about relations between subjective experience and quantitative estimates of time seem also to underlie conclusions reached by other workers. Petrie (1952) found that after leucotomy subjects tended to reproduce demonstrated time intervals as shorter, and to estimate verbally the length of time spent on a given task as longer than before operation. Petrie interpreted this as suggesting that 'time seems to be passing more quickly after leucotomy'. To illustrate the 'universal experience' of hashish users that 'time appears to

be markedly lengthened', Bromberg (1934) has quoted the case of a subject who realized after the period of intoxication that what seemed like several days had in fact been only a few hours. Conversely, Barach & Kagan (1940) have concluded from the fact that under the influence of anoxia time intervals were estimated as shorter than actual, e.g. 3 hours spent under anoxia were subsequently judged to have been 2 or less, that 'the perception of time was slowed'.

These various observations are difficult to interpret because of divergencies in procedure, especially in the methods used for assessing time perception. However, for the case of counting out seconds—one of the commonest techniques and the one adopted in the present investigation—the prevalent assumptions seem to be as follows: If for an individual time seems to be passing subjectively 'more quickly than usual', he will count out seconds more slowly than before, since the interval of clock time which he normally thinks to last one second will now appear to him shorter than previously and he will therefore wait longer before judging one second to have elapsed. Hence his 'corrected' estimate of a second will be objectively longer. On the other hand, if time appears to be passing 'more slowly than usual', the subject will think that time is up when objectively it is not yet up, and seconds will be counted out more quickly than before. The experiments to be reported enabled these assumptions to be tested.

II. Methods and procedure

The experiments were carried out in conjunction with others on the effects of nitrous oxide on various aspects of cognitive behaviour (Steinberg, 1954) and were fitted into a common design and procedure. A simple method of difference design was used involving two groups of 50 subjects each, matched for age and sex, and two experimental trials separated by 1 week (Table 1). In trial I treatment was identical for both groups, and subjects carried out tasks under ordinary conditions. In trial II the drug was administered to experimental subjects through a face mask connected by rubber tubing to gas-flow meters. The concentration was 30% nitrous oxide in oxygen. Control subjects received similarly scented air from the same apparatus.

Table 1. *Experimental design*

Group	Treatment	
	Trial I	Trial II
		Drug
Experimental (50 subjects)	Estimation of seconds (Cognitive tasks) Estimation of seconds: re-test	Estimation of seconds (Cognitive tasks) Introspections Questions
		No Drug
Control (50 subjects)	Estimation of seconds (Cognitive tasks) Estimation of seconds: re-test	Estimation of seconds (Cognitive tasks) Introspections Questions

Time perception in both the experimental and control groups was investigated by the following methods:

(i) *Estimation of seconds.* The subject was required to estimate the duration of 30 seconds by counting aloud at an estimated speed of one count per second. The time he actually took

to complete the count of thirty was measured. In trial I, subjects carried out various cognitive tasks for half an hour after completion of this test and were then re-tested, so that measures of reliability could be calculated. No re-test was given in trial II.

(ii) *Subjective assessments.* On completion of the cognitive tasks in trial II, gas administration was stopped and the subject was asked to write down full introspections on any experiences he might have had during inhalation. This gave him an opportunity to comment spontaneously on changes in time perception. After the introspections had been written, the experimenter asked a number of questions about the effects of inhalation, one of which was whether the subject had noticed anything unusual about the passage of time during the experiment. If he answered in the affirmative, he was asked to specify the nature of the change.

III. RESULTS

(1) *Subjective assessments*

Subjects' spontaneous introspections were searched for references to time perception, and both they and the answers to the experimenter's subsequent questions were classified in three categories, according to whether time during inhalation had appeared to pass (i) more quickly than usual, (ii) more slowly than usual, or (iii) at a fluctuating or unspecifiable rate. This analysis was carried out separately for the spontaneous introspective material and for the answers to the experimenter's questions. The results are summarized in Table 2.

Table 2. *Subjective assessments*

	Experimental group		Control group	
Report on passage of time	Spontaneous introspections	In answer to questions	Spontaneous introspections	In answer to questions
'Quicker'	13	26*	0	12
'Slower'	4	14*	0	10
Miscellaneous	2	7	0	0
No change reported	31	2	50	14
No. of subjects	50	49†	50	36†

* One subject is included whose replies to questions contradicted his spontaneous introspections.
† For practical reasons it was not possible to ask all subjects the questions.

It will be seen that nearly all experimental subjects reported some change in time perception, though only 19 of them did so spontaneously. In their spontaneous introspections 13 experimental subjects reported 'quicker' passage of time, and altogether 26 said 'quicker' in reply to the experimenter's questions; 12 of these had already done so in their introspections, but one subject contradicted his introspections in answering the questions. Four subjects reported 'slower' passage of time in their introspections, and 14 made this report in answer to the questions. Again one subject gave self-contradictory reports. Few subjects gave answers appropriate to the third category of classification. In the control group no subject reported any time changes spontaneously, though in answer to the questions 12 said 'quicker' and 10 'slower'. Since the experimenter's questions produced more responses from the experimental group than had been given spontaneously, and elicited responses from the control group where none had been given spontaneously, it seems possible that subjects were to some extent suggestible. Answers to the experimenter's questions may therefore be less trustworthy than subject's spontaneous reports.

(2) *Estimation of seconds*

The results for the two groups of subjects for each of the experimental trials are summarized in Table 3.

(i) All mean values, including those for measures scored under normal conditions in trial I, are greater than 30, i.e. there was a tendency for seconds to be counted out more slowly than clock time. Individual variation was considerable, however, as is shown by the size of the standard deviations and ranges.

Table 3. *Estimation of seconds: summary of results*

Measures in seconds	Experimental group ($N=50$)			Control group ($N=50$)			Diff. between diffs.
	Trial I	Trial II	Diff.	Trial I	Trial II	Diff.	
Mean	34·98	40·68	+5·70	33·20	38·00	+4·80	−0·90
S.D.	9·06	10·23	—	9·17	10·62	—	—
Range	19–70	23–65	—	13–63	13–71	—	—

(ii) The two groups of subjects did not differ significantly in respect of initial performance in trial I ($t<1$).

(iii) In trial II the means are significantly greater in both groups, as compared with trial I ($t=4{\cdot}27$ and $4{\cdot}66$, D.F. $=49$, $P<0{\cdot}001$). The change of performance in the experimental group from trial I to trial II did not differ significantly from that in the control group ($t<1$). The drug therefore cannot be regarded as having had any effect on estimating seconds.

That the mean scores of both groups should have risen at all may possibly be an artefact attributable to the apparatus used to administer the gases in trial II. Several subjects reported that wearing the mask made their breathing more deliberate and their natural rhythm of counting aloud was sometimes impeded since they could only talk 'in between breaths'. The possibility that some such external factor was responsible is strengthened by the relatively high reliability of the test. Product-moment correlations between initial scores and re-test scores in trial I were found to be 0·74 for the experimental and 0·89 for the control group. The correlations between scores in trials I and II were 0·61 and 0·69 respectively. The smaller size of the latter pair of correlations may be due to the longer time interval which elapsed between trials I and II—one week as against half an hour—and to individual differences in adjustment to the apparatus and the effects of inhalation.

(3) *Comparison between estimation of seconds and subjective assessments*

The finding that the experimental group and the control group do not differ so far as estimating seconds is concerned does not automatically rule out the possibility of a consistent relation between estimations and subjective assessments. In accordance with the assumptions set out in the introduction, individuals subjectively 'slower' during trial II should have counted out seconds more quickly than before, and vice versa. In order to check on this possibility, the differences in estimation times between trials I and II were calculated for the experimental subjects. The mean value of these differences for the 26 subjects who had replied 'quicker' to the experimenter's questions was found to be almost identical with that of the 14 who had responded 'slower', as is shown in Table 4. A similar

result was found to hold for the control group. When the comparison was confined to the 17 experimental subjects who had made spontaneous reports of 'quicker' or 'slower', the result was again similar.

It may be concluded, therefore, that in the present experiments no relation existed between changes in estimating the length of seconds and subjective impressions of changes in the rate of passage of time.

Table 4. *Mean differences in estimation times between trials I and II, in relation to answers given to experimenter's questions*

	Experimental group		Control group	
Answers to questions	*N*	Mean difference in estimation times between trials I and II (seconds)	*N*	Mean difference in estimation times between trials I and II (seconds)
'Quicker'	26	5·46	12	8·10
'Slower'	14	5·50	10	3·75

The difference between the 'quicker' and 'slower' subjects is not statistically significant in either the experimental or the control group.

IV. Discussion

In the light of the results described, the original assumptions cited in the introduction were re-examined. Since it had been shown that changes in subjective experience were not reflected by changes in counting out seconds, it followed that the latter did not constitute an appropriate measuring device for the former. The following reasons are suggested:

First, the assumption that subjective impressions of quicker passage of time should lead to slower counting of seconds and vice versa is logically not compelling, since an equally plausible case may be made out for the opposite relation: thus, if for an individual time appeared to be passing 'more quickly than usual', he could also be expected to count seconds more *quickly*, since it would appear to him that the moment for counting the next second was already there when according to clock time it had not yet arrived. The apparent paradox is probably attributable to the ambiguity of statements like 'time passed more quickly (or more slowly) than usual'; the reference may, for example, be primarily to some sort of 'internal clock', the speed of which is somehow perceived to have altered, or it may be to the rate at which events in the outer environment are experienced. Such a distinction is illustrated by comments like the following: 'Overall time seemed longer—it's hard to describe—I had more time to do the tasks, but time itself was passing more quickly.'

Secondly, one could not be sure in the present investigation of the extent to which estimates of time may have been influenced by objective frames of reference, and of how far subjects may have tried to make corrections. Suppose, for instance, that during the drug administration time seemed to a subject to be passing more slowly, in the sense that he seemed to have more time for a certain task than on a previous occasion, but other cues led him to guess that the objective time must be the same: there then is no reason why he should deliberately try to compensate by altering his usual speed of counting out seconds. He could (but equally well might fail to) realize that although his usual speed now appeared to him, say, slower, it was objectively probably still the same as before and therefore, presumably, correct.

Thirdly, two different kinds of time perception seem to be involved. Counting out seconds requires judgements of the duration of time intervals; the subjective impressions, on the other hand, refer not so much to duration as to rate of passage of time. Conceivably, statements about the former can be made independently of the latter. For example, a time interval may in retrospect be judged to have appeared shorter than its objective duration, yet while it was in progress time may have been experienced as passing 'more quickly' or 'more slowly' or 'at the same rate as usual'.

From the above arguments it follows that any given judgement of time, whether impressionistic or 'objective', may be determined by a complicated interplay of subjective and contextual factors; and there are no theoretical or, as the results of the present investigation have confirmed, consistent empirical grounds for expecting impressions of changes in the rate of passage of time and estimates of durations of time intervals to be systematically related. Nevertheless, investigations like those quoted earlier, in which a relation of this kind was apparently observed, seem worth pursuing further and with larger numbers of subjects and the co-ordinated use of different methods of assessing time perception. It may become possible to identify quite specific aspects of time perception which are selectively susceptible to drugs and other physiological influences. Apart from their intrinsic interest, such findings could be utilized for discriminating between various factors involved in time perception and for studying their respective roles.

V. Summary

Effects of an anaesthetic drug (nitrous oxide) on subjective impressions of changes in the rate of passage of time were studied, and were compared with performance in a test of estimating the duration of seconds by counting aloud. The assumption was examined that subjective impressions of 'quicker' passage of time should be reflected in slower counting of seconds and vice versa. The results showed that:

(1) The drug induced subjective impressions of changes in the rate at which time was passing in a substantial proportion of subjects, some of whom reported apparent quickening and a smaller number apparent slowing.

(2) The drug had no effect on estimating the duration of seconds by counting.

(3) No relation could be demonstrated between subjective impressions of changes and changes of performance in the test of estimating seconds.

Possible reasons for these findings are discussed.

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SOME FACTORS INFLUENCING THE EFFECTIVENESS OF AN INSTRUCTIONAL FILM

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I. *Introduction* (p. 280). II. *Method* (pp. 280–282). III. *Results* (pp. 282–285): (i) *Analysis of sketches* (p. 282); (ii) *Analysis of responses to oral questions* (pp. 282–283); (iii) *Comprehension and assembly performance* (pp. 283–284); (iv) *The factor of past experience* (p. 284); (v) *Reports of film-instructed subjects* (pp. 284–285). IV. *Conclusions and Discussion* (pp. 285–288). V. *Summary* (p. 289). *Appendix* (pp. 289–292). *References* (p. 292).

I. Introduction

The rapid strides in the development of pictorial devices for communicating information and instruction has led to the hope that the moving film and television might come to supplant rather than supplement predominantly verbal methods of teaching and training. This hope appears to be based on the belief that pictorial material makes a greater impact on the learner which is liable to increase in proportion as all features of objects, situations and events, particularly motion, are faithfully represented.

But there is growing evidence that increased fidelity of representation is not *per se* an effective substitute for clarity and soundness of exposition. In a previous experiment (Laner, 1954) no significant differences were found between the performances of subjects on a manipulative task after instruction by either a highly realistic moving film or by a filmstrip composed of line drawings, the pictorial material in the latter being obviously more in the nature of supplementary illustrations. In particular no special effects were found that could be attributed to 'realism' or 'motion'.

The experiment described in this paper attempts to follow up and extend some of these findings. It was designed so as to bring out as clearly as possible whatever special effects might be contributed by realistic portrayal, especially represented motion. Towards this end: (*a*) a moving film was selected showing the operation of a relatively complicated and unfamiliar piece of mechanism, which could only be understood if the interactions of its various parts in motion were appreciated; (*b*) this film was pitted against a corresponding text, supplemented merely by two static diagrams. The subjects, having been instructed by one of the two devices, were required to sketch and explain the mechanism, as well as design their own procedure for assembling it.

It was further attempted to analyse out factors in film communication which appear to influence the perceptual process in specific ways, and thereby contribute to or detract from the instructional effectiveness of a film; to trace such factors and define their effects is among the chief aims of the research of which the present study forms a part.

II. Method

The display material

The two types of display material used contained an identical account of the mode of operation of the Bren-gun trigger mechanism. The main considerations which led to the selection of this content were the great emphasis given to represented motion in the film,

as well as the fact that even subjects who had previously handled a Bren gun could be relied on to be unfamiliar with the trigger mechanism, the dismantling of which is normally an exclusive prerogative of trained armourers.

The film passage, shown individually to one group of subjects, forms part of a complete explanatory film of the Bren gun, produced by Publicity Productions Ltd. for the instruction of Army armourers. During the experimental showings it was always preceded by an introductory passage, which shows the general mode of operation of the whole gun and which provided the necessary context. The whole showing lasted 10 minutes 15 seconds.

The text used to instruct a second group of subjects was also administered individually, and was a literal transcript of the film commentary, except that references to the display and repetitive statements were deleted. A general description of the way in which the Bren works was included in it. Two static diagrams were appended. (For text and diagrams, see Appendix.)

Subjects

The experimental population consisted of fifty male subjects, all University students of whom half saw the film; the other half studied the text. None of the subjects had had any previous experience with the trigger mechanism.

Procedure

As a preliminary to each session all subjects were told of the purpose of the experiment. It was pointed out to them that the initial passages in the film and the text were merely orientational, but that comprehension of this passage would provide them with the necessary context for understanding the trigger mechanism and should therefore be attended to. The text-instructed subjects were in addition told that they would be allowed 10 minutes 15 seconds to study the text and that they were free to make any use they wished of the diagrams.

Testing procedures

Immediately after viewing the display each subject was asked to draw a rough sketch of the trigger mechanism and to insert any names of parts that he could recall. The manner in which the display had been utilized was then inquired into. Next, comprehension was tested by a set of carefully phrased questions. This oral inquiry made the situation less constrained and made it possible to demand further clarification, where the initial explanation was not deemed sufficient. Special care was taken not to give any hints. Eighteen questions were asked, e.g.: 'What is the effect of the Change Lever on the Tripping Lever?', or 'What is the function of the double claw with the gun set at repetition?'

Finally the subject was given the seven component parts of the trigger mechanism and asked to assemble them. The manner of assembly was not shown in the film or described in the text, and the subject had to evolve it for himself. The time taken to complete the assembly was recorded.

Scoring and analysis procedures

The three sets of data obtained were analysed as follows:

(i) The sketches were classified into two categories according to whether the mechanism as drawn could be held to 'work' or 'not work'. To eliminate such irrelevant features as,

for example, artistic merit, the sketches were analysed part by part and points were awarded for (*a*) inclusion of a part, (*b*) 'functional' shape of the part (would it work as drawn?), and (*c*) 'functional' position of part. It was found that drawings with a score of 9 points or less out of 15 did not meet the criterion of 'workability'.

(ii) The subject's responses to the oral questioning fell readily into four categories: correct answer, doubtful, wrong, no answer. A comprehension score for each subject was obtained by summing his correct replies and converting into percentages.

A comparison of the total percentage scores of the subjects in the two groups, however, allows only a comparison of 'overall effectiveness'. By summing the responses of each whole group of subjects to each individual question, it was possible to obtain, in addition, an indication of display effectiveness at each stage. The replies were summed into a 'Right' and 'Wrong' category for each question, the 'Right' category comprising all correct responses, the 'Wrong' category all incorrect responses as well as failures to respond (responses classified as doubtful were left aside). The resulting 2×2 tables were treated by the χ^2 technique.

The times taken by subjects to assemble the trigger mechanism provided scores which could be treated in a statistically straightforward manner.

III. Results

(i) *Analysis of sketches*

The pattern into which the workable and unworkable sketches distributed themselves is shown in Table 1.

Table 1

	Workable	Not workable
Film group	10	15
Text group	8	17

The expectation that the film group would do better on the sketches was not supported by the χ^2 value of 0·34, which does not even approach significance. It should be added that the subjects did not know beforehand that they would be asked to draw a sketch. Their performances tended to be halting and, as far as one could judge, looked like attempts at construction. It is, furthermore, noteworthy that most subjects tended to recall the name of any given part first before drawing it.

Table 2

	Mean % correct replies	Standard deviation
Film group	63·99	29·05
Text group	53·81	34·38

(ii) *Analysis of responses to oral questions*

As described in the previous section a comprehension score for each subject was obtained by summing his correct replies and converting into percentages. The mean percentage scores and the standard deviations for the two groups of subjects are shown in Table 2.

The approximately 10% difference between the means is not statistically significant ($t = 1{\cdot}11$); moreover, as will be shown, the size of this difference is exaggerated.

The overall comprehension scores, on which the above calculations were based, are not by themselves good indicators of display effectiveness. Under certain circumstances—to quote an extreme example—the film might be more effective in conveying the first five facts or concepts and the alternative method in conveying the final five; identical overall scores would in such a case cover up completely the differential effectiveness of the two forms of instruction. By comparing the response of each group as a whole at each stage this differential effectiveness is thrown into relief. Table 3 shows the χ^2 values obtained by the complementary method.

The χ^2 values for questions 16–18 are significant at the conventional levels, but this significance is purely statistical. They are accounted for by the fact that many of the text-instructed subjects found the time too short to get to the end of the description; answers to the last three questions from these subjects were therefore either inadequate, or (for the most part) absent. It is easy to see that this same fact was also reflected in the text-instructed subjects' overall comprehension scores and through these scores in the mean value, thus contributing to the 10% difference between the mean comprehension values.

Table 3

Question	χ^2	Question	χ^2
1	1·44	10	0·37
2	1·44	11	0·33
3	1·44	12	1·71
4	Not calculable	13	3·12
5	1·00	14	0·03
6	1·33	15	1·56
7	0·95	16	7·59
8	0·52	17	7·14
9	1·28	18	9·62

Of the remaining χ^2 values that for question 13 approaches significance, the advantage here being decidedly with the text-instructed subjects. The question involved the action of a spring, which apparently was not perceived at all by many film-instructed subjects, who also missed its functional importance. The spring was omitted from many of the sketches of the film-instructed subjects, while all the text-instructed subjects included it.

(iii) *Comprehension and assembly performance*

Despite the fact that neither film nor text gave any description of how to assemble the trigger mechanism, all subjects succeeded in the task of evolving an assembly procedure. The time taken to assemble varied widely among individual subjects, although these variations had, apparently, little to do with the nature of the instructional device used: the difference between the mean assembly time of the film- and text-instructed groups was only 1·36 minutes and not statistically significant. It was, however, noted that the time taken to complete the assembly tended to reflect two kinds of approaches: a subject who tackled the task in a purely trial and error fashion usually took a good deal longer than one who chose a less random approach. This in turn made it not unreasonable to test the hypothesis, suggested by a previous experiment, that a more adequate assembly performance and better comprehension are connected.

For various reasons the procedure of correlating comprehension and time scores was rejected; instead the two groups of subjects were each divided into two classes, one

comprising all subjects who showed above average understanding on the oral questionnaire, the other comprising all subjects who fell below average. The mean time scores of the two classes in the film and text groups were then compared statistically. The statistics obtained are shown in Table 4.

Table 4

	Mean for 'above average' comprehension (minutes)	Mean for 'below average' comprehension (minutes)	Diff.	t
Film group	5·38	10·55	5·17	3·82
Text group	4·13	9·03	4·90	3·72

The 't' values are significant at the 0·001 and 0·01 levels respectively.

From this table it is quite clear that, regardless of the type of instructional material used, subjects who showed more comprehension of the functioning of the mechanism were also considerably quicker (i.e. better) at constructing for themselves an assembly procedure. Although it would seem to follow that a more adequate performance is based on better comprehension, the alternative hypothesis has to be considered, i.e. that a common factor underlies both comprehension and assembly performance.

(iv) *The factor of past experience*

The term 'past experience' in this context does not denote that the subject already knew how the Bren-gun trigger mechanism works or that he had assembled it on a previous occasion; it must be emphasized that none of the subjects had had such specific experience. A subject was classified as 'experienced' on the basis of his general familiarity with things mechanical, the extent of which was determined by inquiry before each session.

Table 5 shows how the experienced and inexperienced subjects in both test populations compared with one another on the comprehension scores.

There were also differences between the experienced and inexperienced in both groups in the times taken to perform the assembly operation. However, the great variability of the inexperienced subjects precluded a valid comparison of the means.

Table 5

	Mean for experienced (%)	Mean for inexperienced (%)	Diff.	t
Film group	73·00 ($N=14$)	52·52 ($N=11$)	20·48	2·46
Text group	65·65 ($N=11$)	48·80 ($N=14$)	16·85	2·19

Both 't' values are significant at the 0·05 level.

(v) *Reports of film-instructed subjects*

The analysis of qualitative data was in the main confined to reports of subjects in the film-instructed group. Particular interest attaches to those aspects of these reports, which cast light on the stage when the display was actually presented, i.e. when it is making its direct impact. Subjects' reports are, in fact, the only direct means of access to events during the viewing stage, of which the quantitative indices of subjects' performances give at best indirect indications.

In general, the subjects' attitude to the effects and effectiveness of pictorial media of instruction was predominantly positive; but this seems merely to reflect conventional views regarding the merits of predominantly pictorial as against verbal methods of conveying information and instruction. This positive attitude was, incidentally, also shared by most subjects in the text-instructed group.

When asked to substantiate the general view with reference to the film they had just been shown, some subjects suggested that the direct pictorial representation of certain spatial relationships and of the directions of movements of certain parts would give the film advantages denied to, for example, a written text. On the other hand, very few subjects found that pictorial presentation favoured the later re-evocation of specific visual images; in fact, many subjects recognized that what imagery they had had was itself of a constructive rather than reproductive nature; several subjects, for example, stated that they had to substitute their own diagrammatic image-versions for the 'confusing' images supplied by the display.

For the most part, however, the subjects were preoccupied in their reports with the effects in perception of the pictorial information being presented in a continuous series—a characteristic of film and television presentation. A survey of subjects' statements suggested that this type of presentation resulted in the comparatively frequent occurrence of a number of checks and stresses in perception:

(*a*) The meaning or reference of some part of the display was misinterpreted and while an attempt was made to correct for this misperception, further stimuli from the screen could not be dealt with. ('I could not follow at some points and there was a sort of breakdown, due to my interpretation of the film content.')

(*b*) A lag in the perceptual response was brought about by the subject's attempt to reorient himself to a spatial or ideational shift in the display. ('Too rapid as the diagram was twisted round. If you hadn't picked up something, you didn't have a chance to pick it up again.')

(*c*) A temporary fluctuation in attention resulted in what subjects described as a 'loss of the thread'.

(*d*) The perceptual response was checked by some vividly represented or unfamiliar item in the display, which temporarily monopolized the subject's attention. ('Not knowing how "sere" is spelt creates a short distraction and thus cuts across concentration on the film.')

(*e*) The perceptual process was crossed by some motivational influence, e.g. anxiety. ('You develop a lot of anxiety as the thing is racing ahead and this anxiety has a disruptive effect. All the time you are trying to hold on to what went before and missing what is just going on.')

Each of these checks—and the list is probably not exhaustive—resulted, according to the subjects, in a more or less extensive loss of information, which was in many instances cumulative.

IV. Conclusions and discussion

Our present results agree in a number of respects with those of a previous study (Laner 1954) and it is significant that in neither case could any evidence be found for any special effects attributable to the realistic and detailed character of the pictorial representation—such, for instance, as the portrayal of movement. The first study dealt with the execution of a manipulative task after instruction by film or film-strip. In the present experiment the

emphasis was upon the comprehension of a mechanism of mutually interacting moving parts. It would seem that in both of these diverse connexions, the realistic representation of the movements involved was relatively unimportant. Our observations indicated that it could even be confusing.

On the other hand, it seems important for clear perception and understanding that the effects or end-results produced by each consecutive movement or sequence of movements and their relationships to the preceding stages of events or operations should be adequately demonstrated. If these relationships are clear to the subject, there seems to be no difficulty in constructing an adequate version of intervening events, whether in terms of actual movements or in terms of verbal explanation. The fact that in the present experiment those subjects who had shown the best understanding were also quicker and better at devising their own assembly procedure supports this contention.

The dismissal of 'realism' as comparatively unimportant, however, has implications which have a bearing on the still highly controversial issue of pictorial *versus* non-pictorial, and visual *versus* auditory presentation of information. It is widely held that realism is the very factor which makes pictorial visual presentation more efficacious; underlying this belief are two assumptions: first, that the eye can take in more information than the ear; and second, that since a picture presents simultaneously a good deal of information, the superior 'carrying' capacity of the eye can be brought fully into play. But, as many studies have shown, the important factor in perception is not the high carrying capacity of the visual channel, but the very much smaller capacity of central mechanisms in handling the incoming information. Moreover, as experiments with brief exposures of pictorial material have shown (e.g. Bartlett, 1932), very little accurate information can be obtained 'at a glance'. It is significant that studies with films, designed specifically to establish the superiority of visual presentation, have produced negative or at best equivocal results (Nelson, 1949; Nelson & Moll, 1950; Yale Motion Picture Research, 1947; Van der Meer, 1950).

It does not, of course, follow that for certain types of information visual presentation may not have advantages over other types of presentation. It does follow that increased efficiency is not an automatic by-product of realistic, pictorial-visual presentation, but that efficacious pictorial communication is subject to a number of conditions, just as are other forms of communication. These conditions are inherent both in the receiving organism and in the display. In the discussion that follows an attempt is made to elucidate some of these conditions with particular reference to continuous serial displays (film, television). Though the arguments advanced are, at this stage of the research, based in the main on subjects' reports, the fact that their evidence is at many points in good agreement with the findings of research undertaken elsewhere renders the account given somewhat less tentative than it would be otherwise.

Of the conditions contributed by the viewer—these have, not inappropriately, been termed 'audience variables' by the Pennsylvania film research team—the most important in the present and previous studies appears to have been related past experience. Apart from the quantitative results obtained to this effect, there are numerous reports by subjects which indicate that past experience not only made it possible to integrate the information actually present, but provided a framework of future reference, which in turn allowed anticipation. That the possibility of anticipation is a material help in perception has been stressed by many subjects.

Our main concern, however, was not with 'audience variables' so much as with factors inherent in the structure of the display. Judging by the frequency of subjects' complaints and remarks, one of the main factors in this category is the so-called rate of development, i.e. the rate at which the information is conveyed. In practice—and this is where 'realism' comes in once more—this rate is commonly governed by the rate at which the represented events actually occurred; occasionally use is made of slow motion. If, however, visual aids are regarded not as devices for copying reality, but as means for effectively conveying information, it at once becomes clear that what must primarily be taken into account is the rate at which any given information content can be dealt with by central processes. In general this rate for any type of audience lags well behind the actual occurrence of events; representation at actual speed then leads to disruption of the perceptual processes by motivational factors, such as anxiety or apathy resulting from frustrated efforts to perceive what is going on. Occasionally, on the other hand, when slowly occurring events are represented at their actual speed, the perceptual process is disrupted by boredom and tedium. The problem is one of determining empirically broad optimal ranges of rate of development for various types of information. Experimental studies along these lines have already been started (Jaspen, 1948, 1950).

Closely akin to the rate of development is the factor of repetition, which has also frequently been mentioned by subjects. Although it can be considered established that repetition as a whole or of certain sequences tends to increase the amount of material assimilated from the pictorial medium, there still remains the problem of the form repetition should take. In this connexion the information theory principle that the probability of getting information across may be only slightly increased by literal repetition has been strikingly paralleled in Jaspen's (1948) study. It was found that the effectiveness of a film 'key' sequence increased in step with the inclusion of one, two, three and four *non-identical* repetitions. Certain indications were found in the present experiment which suggested reasons why literal repetition may be less effective, the chief being probably the interference of boredom and straying of attention.

A further factor which has been little noted so far, but which appears to influence the intelligibility of the visual message to a considerable extent, is the frequency of shifts. By 'shift' is here undersood any sudden extensive change in the display. A spatial shift, for example, is introduced when a long-shot sequence, giving a more inclusive view, gives way to a close-up sequence, where a small part-aspect of the overall view takes up the major proportion of the total screen area; a similar type of shift is introduced by a change in camera angle. Every change of this kind appears to demand some reorientation on the viewer's part. But in addition the viewer must, presumably, also develop some sort of spatial scheme, corresponding to the space in which the camera was moved. And finally he must adapt to sudden changes in sizes. It seems evident that the more frequently such shifts occur, the smaller the chances of a spatial scheme being formed and the greater the likelihood of disorientation and loss of information. Difficulties of this kind were frequently mentioned by subjects, directly or indirectly. Similar difficulties were reported in connexion with a shift in depicted content—an 'ideational' shift—e.g. at the point where the film switches from a representation and description of the Bren gun as a whole to the representation of the trigger mechanism. Here the ideational and spatial shifts occurred simultaneously.

But by far the most important factor influencing the intelligibility and thereby the

effectiveness of the visual display appeared to be the spoken commentary. Although the use of many unfamiliar technical terms to accompany an equally unfamiliar pictorial content tended to throw into particular relief the more negative effects of the commentary, yet at the same time it emphasized the extent to which the commentary can affect the perception of the visual element. Numerous remarks made by subjects suggested, in the first place, that as a consequence of their inability to get hold of a technical term they were left to face a relatively undifferentiated welter of visual impressions. ('If you knew names of parts, you would know what they looked like and would be able to follow better.') There was no dearth of reports indicating that the commentary, and particularly certain of its key sequences and terms, can materially assist visual perception by contributing to the structuring and articulation of the perceptual field. ('Knowing names would help in organizing'; 'If I had known the names of parts, I would have been less bewildered at the start'.)

Even more importantly, the commentary seemed vital in communicating functional relationships. While a pictorial display may be adequate in demonstrating certain spatial and directional relationships, more abstract functional and cause-and-effect relationships seem to require explicit formulation in verbal terms before they can be grasped. This is possibly due to the fact that such relationships demand the perceptual bridging of time-gaps as well as the narrow selection of relevant aspects from a display where relevant and irrelevant aspects carry equal weight. In the present experiment it was often the *name* of some part which contained the characterization of a functional relationship (e.g. tripping lever). When, then, for one reason or another, the name was not perceived or retained, the functional relationship remained equally unperceived ('. . . missing the name one would miss the function of the part, too'; 'Without remembering the names it was impossible to follow. . .').

Altogether, the evidence converges on the principle that the effects produced by the auditory and visual elements of a combined display—particularly a continuous serial display—are not independent and simply additive. Thus a commentary that overlaps highly in content and formulation with the pictorially presented content can and does materially facilitate pictorial perception. On the other hand, a commentary that has too high a word density (Jaspen, 1948; Zuckerman, 1949), is inadequately synchronized or badly formulated, uses a high percentage of unfamiliar expressions or diverges from the visual element in content is not just ineffective; it tends to reduce the effectiveness of the pictorial element. This is perhaps because instead of smoothing the assimilation of pictorial information it imposes an additional load on the perceptual mechanisms and thus makes the task of handling the incoming information more difficult.

To conclude, it has become increasingly evident to the experimenter in the course of the analysis that from the point of view of assimilation of information the greatest importance attaches to the way in which the message is organized. The problem of pictorial message formulation seems to be, in fact, akin to the problem of 'encoding' which information theory recognizes as being an important consideration with reference to the efficiency with which a message can be handled by the receiver. But since, in practice, visual aid communication almost always involves the simultaneous employment of pictures and verbal and other symbols, and the simultaneous employment of the auditory and visual channels, the problem of pictorial formulation is part of the wider problem of concurrent pictorial-verbal-symbolic formulation.

V. Summary

1. Experiments were carried out in which twenty-five subjects were individually shown a sound film of how the trigger mechanism of the Bren gun works. A comparable group of subjects was given individual instruction by means of a printed text and two static diagrams. Subjects were asked to draw a sketch of the mechanism, explain how it operates and assemble a model.

2. In terms of overall scores no significant difference was found that could be attributed to the effect of either instructional device. Furthermore, neither device was more effective in conveying any part of the instructional content.

3. A significant positive relationship was found between comprehension scores and the time taken to evolve and carry out an assembling procedure.

4. A significant positive relationship was also found between adequacy of explanation and the amount of related past experience.

5. It was concluded on the basis of the quantitative, as well as qualitative, data that realistic pictorial portrayal contributed little to instructional effectiveness. An analysis of subjects' reports suggested a number of factors in the film medium which appear to be more important in this respect.

The continued financial support given this research by the Trustees of the Nuffield Foundation is gratefully acknowledged. Dr M. D. Vernon has readily offered her advice at both the experimental and report stages of this investigation. To Prof. R. C. Oldfield I am indebted for his steadfast sponsorship and interest in the present experiments. Major Reid, C.E.P.R.E., Farnborough, has been most helpful in securing the loan of a Bren-gun model. Mr W. H. Payne, Production Director of the Army Kinema Corporation, has patiently assisted me in the selection of the film and has also helped in obtaining War Office permission to use the film for research purposes. Finally, I offer my thanks to all student volunteers who have readily co-operated in this experiment.

Appendix

The Bren light machine gun

The Bren 303 light machine gun is a weapon which is reliable in action, simple in construction and easy to maintain. The Bren uses ·303 rimmed ammunition.

The Bren conforms to the usual sequence of actions in all automatic weapons, the actions on FORWARD movement being Cock, Load, Lock, and Fire. The actions on BACKWARD movement are Unlock, Extract, Eject, and Feed.

Let us begin with COCKING, the first of the actions on forward movement. The gun is said to be cocked when the pressure of the return spring keeps the piston extension in engagement with the sear, the nose of which protrudes up into a cut-out bent in the base of the piston extension. The sear forms part of the trigger mechanism; pressure on the trigger will rotate it, disengaging its nose from the bent in the base of the piston-extension.

LOADING is carried out when the forward movement of the breech block on the piston extension drives a cartridge from the magazine and pushes it into the chamber.

After loading, LOCKING takes place during continuous forward movement of the piston extension. The cam surface on the rear of the piston extension forces the breech block

upwards. Forced upwards, the breech-block engages the locking shoulder in the body of the gun. Through this action the breech-block is sealed.

With locking completed, the cartridge can be FIRED. This happens at the end of the forward movement of the piston. The piston post, working inside the breech-block strikes the firing pin. This fires the charge.

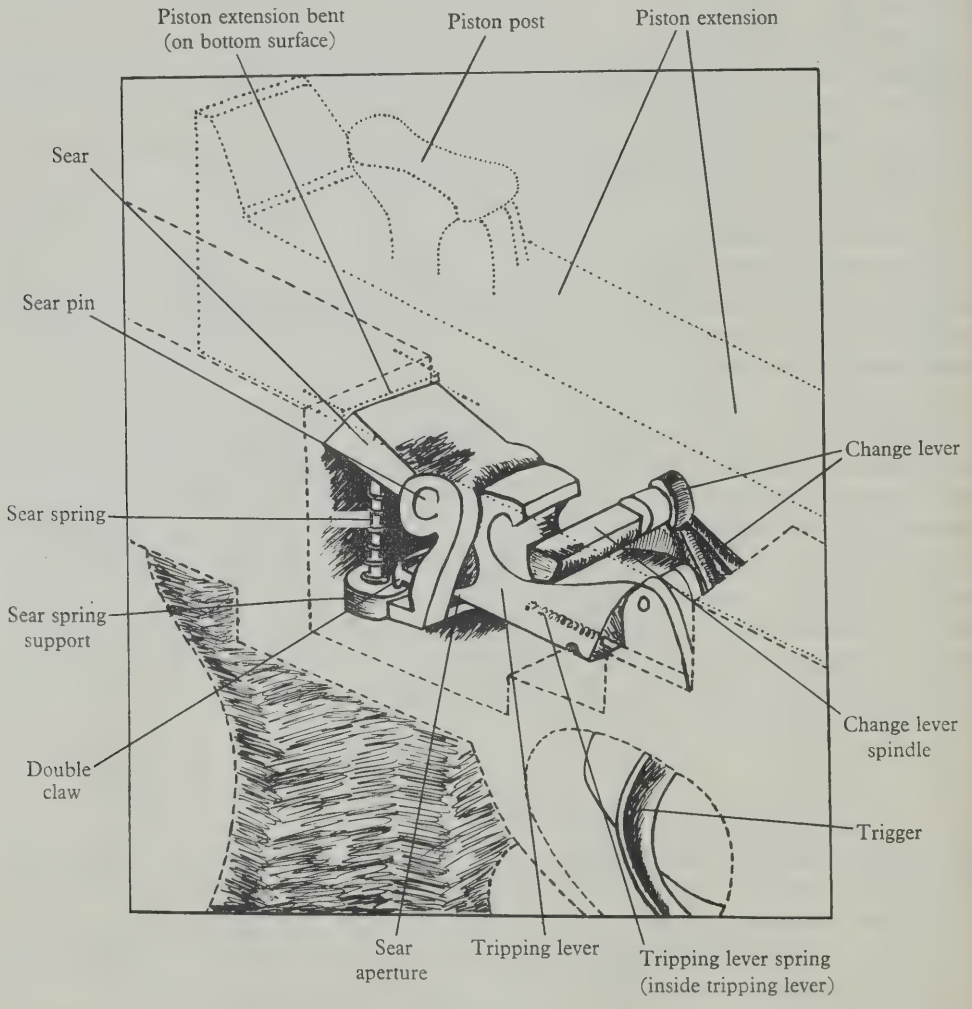

Fig. 1. One of the two diagrams presented to subjects who were instructed by a printed text. The diagram shows the Bren 303 trigger mechanism.

The forward movement cycle is thus completed and backward movement can begin. The power for backward movement is provided by the gases, generated by the explosion of the cartridge-charge. Some of the gases, which project the bullet forward and out of the bore, are diverted through the gas vent into a chamber at the base of the gun, where they meet the head of the piston. The piston is thus driven backwards.

UNLOCKING can now take place. The rear inclined face of the piston post bears against

the interior of the breech block, causing the rear of block to be drawn down, thus unlocking the breech.

With the breech unlocked, EXTRACTION of the fired cartridge is effected by the action of the extractor. The extractor, a small clawlike protruberance of the breech-block, holds the case against the face of the breech-block and withdraws it from the chamber.

In the course of the withdrawal the rear of the spent cartridge case strikes the tip of the EJECTOR. This forces it away from the breech-block and the case drops out of the gun.

With the empty case ejected, FEEDING takes place. The downward pressure of the spring in the magazine first places a cartridge in position.

Meanwhile the recoiling portions continue to the rear completing the compression of the return spring until the piston extension strikes the piston buffer. This completes the backward movement and the recoiling portions move forward under spring pressure to the cocked position.

Although the Bren gun allows for automatic firing it can also be set to fire single rounds like an ordinary rifle. To see how these settings are effected, a closer examination of the trigger mechanism is necessary.

The trigger mechanism

The sear has a spring, pressing it upwards and is connected then to the trigger by the tripping lever. The tripping lever also has a spring, which keeps one end of the lever pressed upwards against the change lever. The other end of the spring connects with the trigger, keeping it pressed forward.

The spindle of the change lever determines the position of the tripping lever. The spindle has three varying surfaces: a large flat surface, a small flat surface, and the rounded surface of the spindle itself. When the spindle is rotated, it will therefore position the tripping lever at one of three different levels: large flat—lever at its highest; small flat—lever central; and round side of spindle—lever at its lowest.

The tripping lever passes through an aperture in the sear and has a double claw. With the change lever set at repetition (single rounds), the large flat surface of its spindle positions the tripping lever so that its upper claw engages the upper edge of the aperture in the sear. Pressure on the trigger will now rotate the sear and depress its nose against the sear spring. However, if now the head of the tripping lever is pressed down, its claw will disengage from the upper edge of the sear aperture. The sear spring will return the sear to its original position. Before the claw can re-engage, the trigger must be released and pressed again.

How does the trigger mechanism control the movement of the piston extension? We saw that in the cocked position the cut-out bent of the piston extension is cocked on the sear nose. When the trigger is pressed, the sear is rotated and thus disengaged from the piston extension. As the piston extension moves forward, its rear will press down the tripping lever head, thus releasing the sear from the tripping claw.

When the piston extension comes back again it first overrides the sear and then, driven forward by the return spring, returns into its original engagement with the sear nose. Every time we wish to fire a round the trigger must be pressed and released again.

Now let us set the change lever to SAFE. This brings the small flat on the change lever spindle into contact with the tripping lever, pressing it slightly downwards and positioning

the claws in the centre of the apertures in the sear. When the trigger is pressed, the claws, positioned in the centre of the aperture, cannot engage the sear and it is therefore impossible to fire the gun.

Next let us see what happens when the change lever is set to AUTOMATIC. The rounded surface of the spindle now meets the tripping lever and presses it further down. When the trigger is now pressed, the lower claw engages the lower edge of the sear aperture. The low position of the tripping lever also removes the head of the tripping lever well below the level of the piston extension. Therefore movement of the piston extension does not affect the tripping lever and the gun will fire as long as the trigger is pressed and there are rounds in the magazine.

Recapitulating then: change lever set at safe, trigger inoperative. At REPETITION the sear engages each time the piston returns. At AUTOMATIC the sear remains depressed as long as pressure is exerted on the trigger.

The description of the trigger mechanism is complete. It is, however, possible to mishandle the change lever and cause a stoppage. If the change lever is set at repetition, or safe, and with trigger pressed it is then set to automatic, the arm of the tripping lever with the double claw is depressed below and in front of the aperture of the sear. On releasing the trigger the claw fouls the base of the sear and the action becomes inoperative.

To start firing again, raise the tripping lever to the middle position by setting the change lever to safe. With the trigger still released set the change lever to automatic. The claws of the tripping lever will now engage the lower edge of the sear aperture and trigger will operate correctly.

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FREUD AND THE CULTURE-PSYCHOLOGISTS

By TERENCE WHITE GERVAIS

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I. Introduction

Two distinct yet related complaints tend to be made about Freud, on which we shall here concentrate. One reproaches him for ignoring 'cultural' and 'social' factors; while the other stresses the limitations of Freud's own culture and environment.

In accordance with the method which uses every psychological concept, theory, or relation as a pattern which can *possibly* be applied to every other, we may employ four main types of analysis. In these we hope fruitfully to apply:

(1) Freud's own patterns and standards of thought to Freud himself and his work.

(2) Those of the 'Socio-Cultural Psychologists' to Freud and his work.

(3) Freud's patterns and standards to the socio-cultural psychologists and their work.

(4) Their own patterns and standards to the socio-cultural psychologists and their work.

II. Merits and defects of both contestants

What we have called the socio-cultural standpoint is a portmanteau term for attitudes of many thinkers, mainly American, but including some Central Europeans. Names such as Horney, Sullivan, Fromm, couple with those of the neo-Adlerian schools; while Jung's critique of Freud touches these at minor points but suggests—if only in certain aspects—a union of the best in both viewpoints. Jung in any case stands apart through his deeper and richer interpretation of the terms 'social' and 'cultural'. Ironically the very words 'social' and 'cultural' are ambiguous, sharing an embarrassing width of meaning. Not only do the various critics of Freud differ in their use of these terms, but each single critic tends either to confuse their uses in his own thought, or else to proffer a single, lopped-off definition in sweet obliviousness of the meanings offered by his colleagues. Hence the display of this diversity, and in a sense the methodological confusion it causes, is *itself a datum* for our discussion. This confusion and diversity can, moreover, be held both in Freud's favour, as showing the unscientific methods of his critics, yet also in favour of a still more serious socio-cultural approach, one which will do justice to the complexity of the facts. The lop-sided interpretations given by several separate critics work, however, still more definitely against the socio-cultural standpoint. Social and cultural, moreover, are themselves often used as synonyms—a confusion all the more dangerous when unconscious.

Yet Freud's critics have some justification. A self-centred, obsessional way of thinking was an inevitable result of the novelty of psychoanalysis and of the insane opposition it

received. Let these critics try to mould a complex set of theories dealing with vital issues, and they might well be more smug than Freud, whose creative fire kept him sensitive to many awkward aspects of truth ignored by the Yes-men. ('Moi je ne suis pas un freudiste', he said to Hanns Sachs.) But these excuses do not destroy the opposition case. Even Brierley (1951) concedes that '...very few psychoanalysts...' have paid 'attention to the second task, that of implications'. Here both sides fall into a parallel error: that of hitting on a valuable aim, while realizing it in a fashion at once confused, limited and distorted. For the culturo-social psychologists themselves fail to see the full implications of Dr Brierley's 'implications'; they interpret the term in a restricted sociological sense, a sense which even 'culture' has to bear, in accord with certain traits of their own age and environment. They have neglected to correlate psychoanalysis with philosophy and religion, and with other attitudes to truth and significance: Jung, however, has here done rich work.

Freud's own position is curious: in his later works he discusses the 'implications' of psychoanalysis, yet with a strange oscillation between admitting he is not now writing as a clinical scientist, armed with precise proof, and claiming that he is so writing. He never resolved the conflict between the anti-philosophy slogans of his 'scientificist' epoch and his own need to write what was in fact philosophy. This confusion remains as a psychological case in the annals of psychological thought. It hampered his thinking on these wider issues, and withheld him from sensitive collaboration with co-workers in philosophic and related fields (much might have emerged from co-operation with philosophically-minded historians). If his bias against philosophy was to some extent academic, that against religion was more vital. By merely *comparing* certain neurotic states with carefully chosen religious or semi-religious phenomena, he claims to shatter man's religious structure. How unscientific a boasted scientist can be! The same treatment, moreover, would with equally false logic shatter morals, philosophy, and Freud's beloved art and clearly essential science.

His cultural limitations come out in a further religious problem: his relative ignorance of all faiths save the Semitic and the Hellenic. Many details, like the 'Father'-obsession, fit still less into Hinduism, Buddhism, Taoism, etc., than into more Western faiths. This is an argument, not only against Freud's views on religion but—in a sense—against the claim that his system is universally applicable. Yet his system may be thus applicable in its clinical and purely 'psychological' aspects: are the individual and family essentially different in the East and in the West? The adjustment of such details could prove fruitful, as also the application of clinically derived concepts to the *differences* between Eastern and less Eastern religions, though without Freud's atheistic rashness.

His uncritical acceptance of the mechanico-determinist urge of his age (one of whose tenets was to explain man by his environment) leads to a stange paradox. We cannot have it both ways, and blame Freud equally for his neglect of environmental factors and for his obsession with the mechanico-deterministic views of his time. The solution to this paradox is, as often, to *recognize degrees*. We can still hold that Freud was restricted *to some extent* by the prevailing *Zeitgeist*, while admitting also that he did, in his theories, appreciate consciously the influence of environment, again *to some extent*. Whether the 'extents' in either case were just and valid is another point; we should be inclined to attribute such validity to the second issue (that of Freud's theoretical admission of environmental influence)—especially if we concede that he was concerned mainly to show

firmly the basis of well-nigh universal psychological patterns. As to the first issue—his restrictions to the current *Zeitgeist*—we must concede this, too, in certain contexts: his determinism (which, again, was partly—though not wholly—a badly expressed *extension* of his necessary scientific method); his Victorian views of woman, and so forth. But Freud was not wholly of his epoch: witness the hysterical opposition to his work, his ignoring of the Hegelian identification of history and thought, or history and morals, and his lack of all naïve 'evolutionary' optimism. This belief in human irrationality and imperfection partly links him with Existentialist trends, though here his scientific balance did forbid him any extremism. As regards his tactlessly repeated formula of man being 'nothing but...' (*nichts mehr als*), it is once more a question of degree: it is futile to insist that Freud *always* said this or that, or *merely* that or this, or took one extreme view rather than another. The inhibition either against holding viewpoints 'in such or such a degree', or against admitting that another thinker has done so, is a vice of our time (from which Freud is not always free): here indeed is a context where the argument of the restrictions imposed by *milieu* and *Zeitgeist* can be applied, especially to certain of Freud's critics.

We can thus partially agree with Thompson that: '...normal self-assertion, i.e. the impulse to master, control, or come to self-fulfilling terms with the environment, was not especially emphasized by Freud', and that: 'Freud's turning away from environmental factors, as a major concern, to organic constitution was, however, most unfortunate. For this reason, he came to minimize what actually happens between people, failed to take into consideration...that it is the dynamic interaction between people which provides the locus of functional mental illness' (Thompson & Mullahy, 1952, pp. 13–27). Once more, we would agree only up to a certain point: for Freud hardly neglected inter-human relations and influences—the whole theory of child-psychology, of trauma, repressing of the Oedipus complex, or the super-ego, disproves such an accusation. A trauma, a repression, a complex, is caused by an event or by events in which usually some other person or persons take a vital part. While Freud's conception of the super-ego is not always as flexible as the facts demand, it remains a recognition of a 'social' and 'cultural' influence. His chief concern, however, was admittedly with individuals and with their effect on each other: his conception of the 'social' lacked the herd-element stressed by many of his critics. Not so easily as many of them did he fall a victim to the identification of more or less intimate personal relationships with larger groups and contacts which alone deserve the full epithet 'social'. Even in dealing with close personal relations, however, he tended towards two limitations: he concentrated perhaps overmuch on relations derivable from or parallel to those clinical contexts from which psychoanalysis takes its origin, and he seemed less aware of the infinite variety of *persons* than of the nature of their influences as reduced to generalized *patterns*.

This preference for 'patterns' over 'persons' we must not, however, exaggerate: first, because Freud himself was often aware of the capricious variety of personal individuality, and because a certain degree of 'depersonalization' is inherent in all science. Certainly psychology is in a different position from other sciences as regards this necessity of method, for its theme is the human person; but the scientist is also a human person—hence a labyrinthine problem of *possible* subjectivism, and of the twistings of the 'Self-hood' of both thinker and theme, to which no other science offers a parallel in any comparable degree.

III. The necessary subtlety and variety of psychological method

We have here reached a point which is inherent in many of our previous arguments. Is there not a salient difference between explicitly ignoring the importance of socio-cultural factors (or even denouncing their importance), and merely stressing the personal, familial, and sexual factors which must in any case prove the *basis* for all fruitful discussion of the socio-cultural field itself? And this, in the main, is what Freud has done, though not always adequately. Historically his approach was, on the whole, the only suitable one for the nascent science of psychoanalysis. What is important is not to regard the two approaches as opposites: too much thinking is done (and sometimes by Freud himself) in naïve oppositional terms. Not every situation is best analysed into a *duality* (though here again Freud was a frequent offender: witness his too numerous antitheses of instincts, etc.).

Of course everyone will draw the line differently between methodological necessity and its extension into an overwhelming doctrine; the method cannot help being three things: a method as such; a part of the very subject-matter; and, to some extent, a potential doctrine. As regards this potentiality of doctrines must we not remember that different thinkers will each tend to regard a method as having become a doctrine or not, in different contexts, according largely to the psychology of these thinkers themselves? If Freud, for instance, is deemed too 'subjective', each of his critics will still act according to his own subjectivity in attributing such a quality to Freud in a wide variety of contexts and degrees. So the 'psychology of the psychologist' hunt, while a most essential aspect of a full scientific method, provides its own problems, and apparently insoluble circles. The whole problem of the inter-relation of a method both with subject-matter and with doctrine is, moreover, one facet of the vast pattern of 'Selfhood' (*Selbstheit*), of Presuppositions, Identities and Tautologies. This asks: what is the relation between anything, fact or idea, and any of the following: its existence, its expression, its emphasizing, its parts or aspects, its relationships, its 'defence', and our consciousness or not of it? Freud cannot be blamed for such a ubiquitous pattern of life's dilemma. And this applies, not only to the relative ignoring of the social factors in favour of basic individual patterns, but equally in the cases of 'depersonalization' and of 'determinism'.

Moreover, the determinism, the depersonalization, or the individualist stress may or may not overstep its valid sphere, but seldom entirely so: hence the need to think in terms of degrees, and also of particular contexts. Here, by the way, is a sphere where the methods of Statistics may be of great use to psychology; or rather, perhaps, statistical methods and statistical 'mentality'. Instead of claiming the supreme (if not sole) validity in psychological method, let these act as shrewd discerners precisely of the *degrees* of fact or validity, and of the exact *context*, to which such-and-such a theory or critique may apply. An unusual role, this, for statistical mentality and method, but surely a useful one: at once a subtilizer and a safeguard.

Another valuable principle is what may be called the pattern of the 'Two Ends'—by which each term in a two-termed situation may be considered, according to the context, as the origin, means, or aim, or as the cause or effect of the other, and the general situation described equally well with either term as the main factor. This pattern (which is of course not universally applicable) can often save us the futile effort of warring one-sided dogmatisms: in art, for instance, with factors such as unity and variety; restraint and

intensity; a 'straight line' aiming at a final 'curve', or vice versa; passion and serene form; individual and 'typical' forms; subjective and objective elements, etc. Similarly, in psychology. Can we not agree with Fr. Victor White, that: '...even Professor Flugel must assume (for he cannot possibly prove) that "God" is a projection of the super-ego rather than allow that the super-ego might be an unconscious introjection of "God" (or at least of certain divine images and attributes)'? (White, 1952, p. 20). In other words, we have here a pattern of the 'Two Ends', which may well fit also, *mutatis mutandis*, into the problem as to the primary of 'individual' or 'socio-cultural' factors (or even as between 'socio-cultural' and 'sexual' factors). If Freud has not seen this, neither have most of his opponents.

Another too sharp antithesis is that of 'subjective' versus 'objective' methods in psychology. We have already shown how, in psychological as against other forms of science, the subjective forms a large part of the objective theme and data themselves, and how an awareness of such subjective data is essential to the psychologist, who—to complicate matters further—adds to the labyrinth of his own subjective slant. Hence, though complete 'objectivity' is the scientific ideal, in psychology at least this will involve a strong subjective element, both an awareness of subjective data and a subtle use of one's own subjective attitude in discussing such data. The 'discovery of general laws of Mind' (objective) and the 'description and understanding of the unique undivided personality' (subjective) are both needed; their results can '...be correlated, and can be used to correct each other' (Pear, 1948). From another angle, 'What is experienced as meaning can be objectively described as a functional integration of mental processes' (Brierley, 1951, p. 94). Why, now, trouble to agree whether the one type of psychological method is either (*a*) 'prior', or (*b*) 'superior' to the other? In such a case, the acceptance of the 'Two Ends' both clarifies and enlarges our approach. On the whole, we find a greater awareness of such breadth of method in Jung than in Freud.

IV. Boy and girl in Oedipus situation: part and whole: degrees and contexts

Freud's work, however, is full of patterns of thought and fact, some of which can even—by a sleight of method well adapted to psychological science—serve to indicate solutions of wider psychological problems. Can we, for instance, not use his distinction (a subtle piece of analytical insight) between the *boy's* and the *girl's* early routes through the Oedipus complex and other familial relationships, as a pattern which can help analogically to 'solve' the dilemma of 'individual' versus 'socio-cultural' approach, or that of 'subjective' versus 'objective' methods?

Freud shows how the pattern of child-parent relationship differs in the case of a boy and of a girl—the girl's relationship having an *extra* stage: the Oedipean attraction of the boy to his mother, and of the girl to her father is preceded in the latter case, by the girl's attraction to her mother. Hence the girl has two stages to the boy's one. Is it too fanciful to suggest the analogy that we shall regard the 'individual' and the 'socio-cultural' approaches, in *some* contexts as methodologically 'equal' (like the earliest relations of both girl and boy to the mother, or their later respective attachments to parents of the opposite sex) and in *other* contexts—probably the more important kind—as conforming to the 'unequal' pattern just mentioned? In other words, let us realize that 'socio-

cultural' psychology *involves* a 'previous' stage of individual psychology, and that this stage, moreover, is not packed into the former and wholly absorbed, but continues ceaselessly to act and react on the very texture of the socio-cultural factors and analyses themselves. Similarly, we may sometimes treat the 'objective' and 'subjective' methods as methodologically 'equal', but also realize the 'twofold stages' in the 'objective' approach, which contains a 'previous' subjective phase which continues its influence. To a slightly lesser degree we may say the same the other way round, that subjective method contains an objective phase; in which case, we have yet another example of the 'Two Ends' pattern, and also of the need to perceive and admit *degrees*.

At last we may seem to have reached a logical and fruitful *modus vivendi* between the 'individual' and the socio-cultural approaches. Yet the accusation cannot be ignored that Freud—or any other psychologist—may have called the part the whole and thus limited his own approach and conclusions: this accusation is indeed made *from and against all sides*.

Thus, Freud on Adler: 'From a highly composite unit one part of the operative factors is singled out and proclaimed as the truth' (Freud, 1918; English trans. 1933, pp. 526–7).

Yet Thompson says of Adler: 'Adler was the first one to point out that sexual disturbance, instead of causing neurosis, was but one of the situations where neurosis manifested itself' (Thompson & Mallahy, 1952, p. 159).

We indeed seem in the presence of some hyper-flexible inside-out indiarubber of the psyche, where, to some degree and in some contexts, *A* may be a part of the whole *B*, while, in other contexts and again to some degree, *B* may be a part of the whole *A*. In psychology, as yet so unformed a science, this may not be so absurd as it seems. The pattern of the 'Two Ends' may legitimately be applied here, though with certain caution. Notice our reservations, 'to some degree and in some contexts'; we may further apply these reservations to the validity of the 'Two-Ends'-pattern itself as regards this problem. For this pattern is by no means the sole one in which the problem can be stated, let alone clarified: we may, for instance, well consider it in the light of an extended pattern of interacting Wholes, *Gestalten*, and of different levels or degrees of micro-macroscopic scale. Moreover, the ubiquitous design of 'Selfhood' and Pre-suppositions, to which we have already referred, appears again in this problem; for so much depends on the pre-suppositions made by both Adler and Freud in their major terms and theories: 'Sex' in the more ordinary sense is certainly *a part* of the wider causes, and aspects, stressed by Adler and Jung; yet, in Freud's own sense, it *includes* many of these, and in this way Adler's 'Power'-theory may seem a distorting extraction of a single element. Yet Adler in turn, in opposing his master's 'narrowness', pre-supposed a number of important factors in the world, which he assumed he had sufficiently brought to light in outlining his alternative concepts. In both cases, certain aspects of life are assumed, and either not mentioned explicitly or else *included* in some wide concept, an arrangement, however, which does not fit in with some other thinker's assumptions, inclusions, conscious and unconscious use of terms and so forth. In these cases we really do seem to have a design of interlocking 'Wholes'; the interlocking, however, is not in the least simple; it is a twisting in-and-out with continually changing emphasis and validities.

Yet none of these solutions may entirely satisfy us. Both in this Freud–Adler contest and in that between Freud and the socio-cultural standpoint, we surely need a method which will sift out clearly *to what degrees and in what contexts:*

(1) The 'Two-Ends' pattern is valid. (This would seem to be the case, in the main, in the discussion whether anxiety causes neurosis, or neurosis anxiety—a point on which Freud himself fluctuated.)

(2) The patterns of interlocking or otherwise related wholes and *Gestalten* and of different levels or degrees of micro-macroscopic scale, are valid, or at least useful and suggestive.

(3) The pattern found in the boy-and-girl Oedipus histories is valid, or at least suggestive and useful.

(4) None of the above really meet the problems. One attitude or the other (i.e. Freud's or Adler's, in the one case; that of Freud or that of the socio-culturalists, in the second) must have the victory.

Research into such questions of degrees and of contexts forms surely a valuable, and fairly novel, type of psychological method.

V. PSYCHOLOGY AND THE PSYCHOLOGIST'S PERSONALITY

In contrasting Adler and Freud we have touched on yet a further problem: the conflict of personalities within psychology itself. This is a result of that *mélange* of scientist and subject-matter in which psychology is unique. Every psychologist tends from time to time to criticize his opponent's views on the grounds of the latter's own personal psychology; yet he will deny doing this, and strenuously object when the retort discourteous is made to him. Then it becomes 'unscientific'. Again it is difficult but imperative to find the *juste milieu* between ignoring and over-stressing the (probable) subjective motives for any psychologist's opinion. Many objections to Freud's theories, based on what was supposed to be his personal character, fail in this respect; so also do many of Freud's own replies to criticism, though here the inane and insane nature of the world's first reaction to Freud provides an obvious extenuating cause. Such an excuse, however, scarcely applies to his attitude toward Jung and Adler; here he seems to indulge in an attack half savage, half tired-ironic, on the inner motives of the 'renegades'. Yet when Adler suggested that the founders of psychoanalysis be analysed, and analysis applied to the creation and holding of analytical theories, this was regarded as lèse-majesté! Moreover, does not Freud, in his *Autobiographical Study* (1935), confess to a power-complex, to jealousy, and to the periodic need to quarrel with a close associate? Yet such factors do not affect Freud's own positive theories and insights.

Ernest Jones, defending the work of Melanie Klein but analysing the opposition it aroused, describes the facile instinct to place her novel theories in the same category as those of Adler, Jung, Stekel and Rank, who were 'influenced by subjective motives—a rationalization of inner resistances.... Yet, if psychoanalysis is to remain a branch of science it is evident that...advance beyond the limits he (Freud) reached is inevitable...' (Jones, 1948). As if Jung's mentality can ever be a more subservient derivative or decoration of Freud's! The ignorance displayed here of Jung's *type* of work, so often complementary rather than opposed to Freud's, is embarrassing but typical: is there no 'rationalization', in Dr Jones's own case, of some 'inner resistance'? May we not extend his last sentence, to the effect that 'if *psychology* (not merely psychoanalysis) is to remain a branch of science,...advance beyond the limits he (Freud) reached is inevitable'?

Psychology is wider than psychoanalysis.

VI. Freud's cultural environment: his view of woman

Let us turn to Freud's own cultural environment and attainments: we feel that these are less wide than Jung's, but far richer and broader than those of most anti-Freudians.

In his *Autobiographical Study*, Freud rebuts the Sorbonne professor's gibe that 'psychoanalysis is alien to the *génie latin*', by asking if it were the favourite child of the *génie teutonique*; and his irony 'defends' Vienna from the charge of contamination with his work. Yet the Viennese *milieu* may have stimulated him, with the 'psychological' and cynically introspective slant of its literature, with its melting-pot of nations and types, with its blend of neurosis and gay charm, its relatively free expression both *of* and *about* sex, and the *Schlamperei* which bred a half-tolerance lacking at that date either in Protestant lands or in the full, sharp Catholic South. Precisely the 'universal' traits in Vienna helped Freud indirectly; 'localist' criticism is thus in error.

Behind these reproaches lay an often subhuman antisemitism. Freud, for all his wide disinterestedness, curiously agreed with his insulters in declaring: 'It is, perhaps, no coincidence that the inventor of psychoanalysis was a Jew', stressing the Jew's acquired power of standing alone. He might have added an absorption in, yet detachment from, prevailing cultural forms, the ability to sift the best in each of them, and to embrace the most diverse facets of the human spirit. Perhaps less admiringly we might point to the overwhelming role of the Jewish family- and Jehovah-traditions in Freud's extreme stressing of the *Father*-complex; while his blind negation of religion may be due in part to this rigid faith, and his rejection of it in the fire of his scientific knowledge. Towards Christianity he felt an ambivalence of intellectual interest and emotional repulsion, while Oriental faiths seem closed to him. Thus it is hard to explain Fromm's statement that Freud's work was the typical result of a 'Protestant environment'; that religious environment was Jewish and Catholic. Why thus bolster a case at the cost of obvious distortion?

No less and no more valid than critiques against Freud's Jewish background and nineteenth-century scientific rationalism, were his own references (especially after their quarrel!) to Jung's Protestant-pastor background and to Swiss mentality in general; or his attacks on America, through partly well-built fears lest it cheapen his psychoanalytical creation; while in and after his lectures at Worcester, Mass., his first public exposition outside Central Europe, and first official welcome in any University, he saw little in America to admire. A grudge-bearing lack of gratitude, indeed not without causes from his hard early life, is hinted at in the *Autobiographical Study* itself.

Of greater strength are the critiques which expose Freud's limited view of woman; even according to Brierley, it is the weakest part of his work. Witness his Victorian views, his uncritical acceptance of the male 'order' of our culture, his equation of the ideas that woman is *different* and that she is *inferior*, his frequent confusions between the 'masculine' and (what is thereby meant) the 'human' ideal; his references to criticisms made against women without any account of criticisms levelled at men; his assertion that child-bearing, house-slaving 'enigmatically realistic' woman is 'less ready to submit to the great necessities of life' (=men?); or the blank equation of woman's subjectivity with a lower ethical level, omitting reference to the kindred feminine virtues. Nor can he conceive that certain female traits are due to age-long social conditions imposed by males. Here we can definitely accuse him of underrating or ignoring the 'social' aspect of psychological fact. He talks of 'feminine vanity', ignoring the male kind. To a female psycho-

logist (and to most women) this lacuna itself shows male vanity. Yet he refers to the mother-son relationship as 'supreme', and we know from his own words how strong was his bond with his mother. (All this is not to deny his many discoveries in female psychology, e.g. penis-envy—though women psychologists like Deutsch and Horney (and even males like Jones) have often been able to adjust certain details.)

These arguments as to his 'masculinity-complex' he would combat by saying that 'the use of analysis as a weapon of controversy obviously leads to no decision' (Freud, 1931, English trans. 1950, p. 258): in other words, the masterful male can apply the 'personal' argument to his female and feminist critics, who cannot return the compliment. His own use of such a method (as applied even to males like Jung, Adler, Rank, Stekel), and his dismissal of it as 'unscientific' when used against himself, might even show—on his own terms—a 'feminine' trait, rather than the impartial male Olympian search for truth.

Truly it is difficult to draw a precise line between ignoring the value of this 'subjective' method; in dealing with woman, however, Freud does not even attempt the task. Similarly, it is hard to draw an exact line between a *recognition* of the masculine bias of our society, especially when this touches female development, and a *psychological theory* based on the assumption that such a bias is inherent in the order of things. On the whole, Adler and Jung both emerge from this test with finer sensibilities than Freud.

This is perhaps the sole exception—judged by his own standard of piercing insight—to the liberal air which pervades Freud's work. His maligned epoch, with its urbane civilizedness and growing social conscience, tinges this work with its 'Live and let live': is this inferior to our strange blend of increased specialization and mania for the 'totalizing' of all activities—even of those ideals of intellectual truth, which in Freud's day stood unquestioned? Harsh pragmatism, either of political ideology or of (partly valid) 'total social criteria', is no advance on at least the attempt at scientific integrity. Hence, by the culture-psychologist's own criterion, we can judge Freud—though but partially—through his cultural air and aim: and these were in many ways more humane and more scientific than our own.

VII. AMBIGUITY OF THE TERMS 'SOCIAL' AND 'CULTURAL'

Perilous ambiguities, moreover, beset the very terms 'social' and 'cultural'. Each is used in a variety of meanings, to suit the context, and furthermore 'cultural' and 'social' are confused with each other. Such laxities cause error and misunderstanding.

Psychologists and non-psychologists alike confuse the term 'culture': witness the debate whether culture should be primarily 'social'—a nice picture of co-operative redoubled muddle. 'Social' and 'political' are also identified, because in *some* contexts they may bear similar or parallel roles. Adler himself uses 'social' now to mean political, now merely gregarious, and now as regards intimate relationships. His system, despite its many insights, overstresses the 'social' aspect, due partly to Adler's own belief in socialism, and even to the word-play contained in the term: here we must to some extent use the social criterion in judging the psychological theory of social adjustment itself. Again, 'The inferiority complex. . . finds its explanation in the relatively greater deficiency of social feeling' (Adler, 1936, p. 115): here 'social' bears yet another nuance; but such nuances are not recognized, ordered, or allowed for. (Cf. also Brachfeld (1951): '. . . as Adler said, sex is not just a private matter'.) As to 'culture', must we assume that Freud

ignored the Greek art and thought enshrined in Christianity, when he described the latter as 'a cultural regression as compared with the older Jewish religion'? (Freud, 1939, p. 142). In other contexts (cf. our later discussion of Incest) the anti-Freudians fall into still louder muddles of 'cultural' terminology.

Timely is Lindner's attack (1952) on the 'adjustment'-mania which destroys adventure and protest: yet here the culturo-social aspect strangely intervenes, for 'social adjustment' is more deified in Dr Lindner's country than in Europe. Moreover, the 'social' tinge common in American 'individualism', the stress laid on a person's 'fulfilling his self' in the interests of a healthy society of similarly fulfilling persons, accounts for the unconscious intrusion of 'social' proposals like Dr Lindner's crusade for eugenics.

Another slant on the socio-cultural aspect of psychology appears in Jung's sense of *the continued coexistence of various cultural attitudes of the past* ('There are people who, psychologically, might just as well have lived in the year 5000 B.C....', while 'a few of us belong to the 3rd or 4th millennium A.D....') (Jung, 1952). This contradicts the facile assumption that every 'main region' of the world has, at a given time, *one* 'culture'. This defect both of vision and of method is parallel to that which speaks incessantly of 'society': as if there were not (even for each individual) myriad interlocking, and often conflicting, communities or groups that link each of us with other humans (even a relationship *à deux* or *à trois*, or even that between a dead scientist or artist and all who benefit from his work, can still prove a potent 'society').[1] How perilously vague, then, to assert that 'art must reflect society...social values must be paramount....Psychology should stress the social angle, and is correctly a branch of social studies'. *We must, in fact, 'socialize' the idea of 'society' itself: this is a methodological parallel to applying Freudian analysis to Freud's own theories.*

VIII. Sexological aspects

Relevant to the socio-cultural argument is Freud's sexology. His emphasis on sex has many facets and causes: scientific precision; reaction against the anti-sex conspiracy; the difficulty of expression in a new science; and the general *Zeitgeist* of an era at once romantic and determinist-materialist. In that era we still largely exist; it is a half-truth to explain away Freud by the sexualism of his times, for that sexualism has not abated, and we should beware of rationalizing an inner resistance to our own natures. It is inaccurate either to play down the sex-motif (like many modern psychologists), or to regard sex-interest as 'the typical neurosis of modern Western man' (Jung, 1928, p. 34): as if other ages of the West, or any Eastern epochs, have not shown similar traits! Hinduism offers but one of many potent examples. The low role of women in most Oriental cultures to some extent confirms, as well as complicating, this point; while their seclusion has as both cause and effect an intense sexual valuation, curiosity, and jealousy; and these are found to some degree in the relative segregation of sexes in Southern Europe, where surely sexuality is as active as in the North. All this implies that we should examine afresh every *detail* of sexualism, which may well differ in various *milieux*: yet not enormously enough to upset the basic teaching of Freud.

Again, the full or partial seclusion of women is balanced in many lands by a more

[1] For more extended treatment of this problem see my article 'Render unto Caesar' (Gervais, 1946) and poem *Patrick Freed* (Gervais, 1948).

general homosexuality. To ignore this facet shows an unscientific prudery, a semi-neurosis from which Freud was free. To him homosexuality was as much 'sex' as the more normal form, not did he judge its importance or values by such criteria as social adjustment, benefit to 'the' community, wholesome or unhealthy moral tone, or military effects. His frank analyses of homosexual elements aroused unconsciously still more opposition than his other sexual doctrines. In this he is more objectively scientific, and less conditioned by culture and social environment, than his opponents who blame him for being too preoccupied with sex. (He himself might have analysed this very resistance in terms of an unconscious homosexuality.) Nor—to mention one facet of a vast problem—can we ignore the (anti-) homosexual complex so evident in the United States and in Britain, to explain the attitude of many socio-cultural psychologists to homosexuality.

Let us therefore examine *in what details* the various Eastern civilizations differ on this point from each other and from the West. (Consider, for example, the relation of the Oedipal theories to China's extreme family tradition.)

Sometimes, however, an important part of Freud's system is deemed inapplicable to either East or West. A salient case is the Incest problem—where the ambiguity of the term 'culture' only adds to the confusion, alike of data and of deductions. Do the widespread laws against incest indicate a 'natural' abhorrence of it, or rather an almost panic prohibition of a 'natural' desire? We shall cite some passages to show *two* types of confusion: (i) that in terms like 'culture' and 'intelligence'; and (ii) that involved in any attempt to generalize human history in such complex problems.

Dalbiez (1941) states, for instance, following Malinowski: 'The family is the cradle of culture.... Hence...incest is seen to be unreconcilable with the very existence of culture.' He then quotes Malinowski: 'Culture in the form of law, morals and custom—forces the male...to keep guard over the pregnant woman.' Is this the 'culture' implied by learning and the arts? Is the Eroica Symphony or the Relativity theory irreconcilable with incest? Even apart from these questions, we dispute the naïve identification of 'culture' with certain types of culture—whereas in others incest has officially flourished (e.g. often as a safeguard for the family unit!).

Dalbiez, again, in equating 'morality' with sexual morality, is more sex-ridden than Freud, who merely showed the sexual origins, and the continued quasi-sexual aspects, of phenomena not usually classed as such. Dalbiez, while accusing Freud of not going deeply into these questions, himself ignores such dilemmas as the collisions, in certain contexts, between sexual custom and other moral laws, like charity and imaginative understanding (the second-grade treatment of 'illegitimate' children is 'rooted in man's human (as distinct from his animal) nature'). 'Cultural sexual morality'—as if persons of finer aesthetic and intellectual culture were not often freer in their sexual opinions!—becomes the unanalysed fetish. Yet again, if all cultures have imposed restrictions on sex, that does not mean we can or should not urge *certain* adjustments or the removal of *some* restrictions. Moreover, each culture tends to a different sexual-moral programme: there simply has not been any such thing as one sole official attitude towards the following aspects of sex:

Monogamy, polygamy, polyandry, homosexuality in males or females, divorce, birth-control, abortion, bastardy, marriage between cousins or to a deceased wife's sister, or masturbation.—Parochialism and lack of elementary logic can go no further.

IX. Conclusion

'Social' and 'cultural' are thus perilously ambiguous. Yet, judging the culturo-social psychologists by their own standards, we may suggest that, if Freud is to be blamed for adhering to the materialist determinism of his epoch, so should these particular critics bear censure for being unduly influenced by *their* time and place, by the sociological craze, which—like the materialism induced by the first flush of scientific conquest—represents the exaggeration (and thus distortion) of a necessary trend. We admit the worth of the 'social' and sociological trends; yet their excessive encroachment is a defect of our time, to be corrected and compensated for with finesse, by present as undoubtedly by future thinkers.

Notable, as we have seen, is the meaning—at once mechanical and vague—given to key-words like 'society' and 'culture'. Notable, too, is the ambiguity between the meanings acted on by different thinkers, and produced unconsciously by diverse environments: for instance, the tendency (not confined to America) to identify the individual's free self-fulfilment with 'constructive service to a progressive community', colours many attacks on Freud: as if such could possibly be the essence of self-fulfilment and freedom, though it is a fruitful corollary, opportunity, and side-product. Yet these same critics (encouraged by some looseness of Freud's language) will blame him for regarding the tendencies repressed by the super-ego as in themselves necessarily bad; while we wonder what use will be found for these often desirable traits by the society of Fromm, Sullivan and others, with its background of vague idealism plus ruthless efficiency: is 'functional social utility' to be their most admired aspect? Freud at least was too 'cultured' to regard the supreme test of art, science, or morals as lying in their social value as defined by social-minded social psychologists. Neither did he restrict 'society' to one specific form, while he gave frank warnings on the nature and conduct of groups as such. A final suggestion: can we find any relevance in Zilboorg's critique (1938) of the overstressing by American psychologists of *anal* reactions, which Brierley reinforces by noting British *oral* preoccupations (Continental Europe is of course needed to complete the picture)?

Yet there are many ideas of these critics of Freud which we can, in general, accept—though, like Sullivan's theory of 'Interpersonal Relations' (1947), they can often be adjusted to the basic Freudian structure. Once more the elusive patterns of what is basic or presupposed, and the relations between method and doctrine! Moreover, in the widespread habit of posing *additions* to the Freudian basis as a *contradiction* of it (a habit encouraged by the most fanatical Freudians), is there no psychological current, conscious or not?

Part of the socio-cultural thesis may be due to an unconscious frustration and embarrassment at the dogmatic 'one-method' attitude exhibited by each rival school, and at the mania for describing man as 'Nothing but...' (*nichts mehr als*): a dogma deplored by Jung but often evinced both by Freud (often through verbal carelessness) and by the culturo-social schools. Against these tendencies, there may arise a semi-articulate urge towards the needed breadth of psychological method—in which psychoanalysis, Jung's system, *Gestalt*, and socio-cultural approaches will go hand in hand with each other and with experimental and statistical methods. If Einstein (1949, p. 684) is right in depicting the physicist as, in different facets, a realist, idealist, positivist and Pythagorean, how much more valid is such a synthesis for the complex, immature, and objective-subjective

science of psychology. In this way, we curiously *sublimate* the culturo-social thesis by bringing the variety of sociality and the breadth and nuance of culture into psychological method itself.

X. SUMMARY

Freud is partially guilty of the twin sins of omission charged against him: his own cultural limitations, and his ignoring of culturo-social factors, while in some respects he shows these factors in a subtler and deeper light. Moreover, these charges have been exaggerated to a point which has led us to expose *comparable* faults in his opponents, who, on the whole, betray grosser defects in these matters than Freud himself: and all this, quite apart from the countless achievements to his credit in other branches, aspects and contexts of psychology.

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A STUDY OF RACIAL ATTITUDES DURING AND AFTER THE WAR BY THE THURSTONE TECHNIQUE

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I. *Introduction* (p. 306). II. *Analysis of the attitude scores toward Germans, Japanese and Maoris* (pp. 306–308). III. *Comparison with a study by Remmers & Wood* (p. 308). IV. *Summary* (pp. 308–309). *References* (p. 309).

I. Introduction

Before the last war the writer had been doing some work, much of it with his class in experimental education at Auckland University College, exploring the usefulness of Thurstone's method of measuring sentiments or attitudes. For some years three attitude scales (towards Germans, Japanese and Maoris) had been used with the class for demonstration purposes. The scales had been constructed on the lines laid down by Thurstone & Chave (1929), most favourable attitude being placed at the zero end of the scale.

When the war started it appeared that there was a good opportunity of checking the validity of the technique by noting whether the attitude towards racial groups with whom we were at war changed in the expected direction, while the Maoris appeared to offer a good 'control' as against the Germans and Japanese. Unfortunately, the results of testing before the war had not been kept, as the work was of a general demonstration type, not conducted with any particular aim. Consequently, the results available for this study are those of 1940 and succeeding years. Testing was done each year, usually in the month of August, near the end of the winter term. The class used was that in experimental education with similar membership in the different years.

II. Analysis of attitude scores toward Germans, Japanese and Maoris and their variations

Table 1 presents the facts of the analysis, giving the fluctuations in the mean attitude scores. The number of cases in each group varied from 20 to 71. The mean scores of the different groups can be roughly interpreted with reference to an approximate indifference score of 5·1 which corresponds to the statement '...neither better nor worse than any other people.'

The most obvious fact of the fluctuations in attitude toward Germans is a rise toward a less favourable attitude in 1945, 1946 and 1947, that is, at the end of the war and for two years after it. The score has fallen considerably in 1950, and by 1953 it has reached the indifference zone. By this time the attitude toward Germans has returned to a normal pre-war level. The main differences in attitude, those of 1945, 1946 and 1947 when these years are compared with the low level of 1942, are the only ones with any degree of significance. The figures of Table 2 show clear significance for 1946 and 1947 and only slight significance for 1945. The unfavourable nature of these attitudes is presumably related to the disturbing and horrifying effects of the revelations of atrocities, concentrations (e.g. Belsen) and other circumstances observed after the cessation of fighting.

Study of the attitude scores with reference to Japanese shows a somewhat similar trend to the German scores. After 1941 all the values lie in the unfavourable zone (above 5·1), those for 1943, 1945, 1947 and 1950 remaining at a fairly high level. Even the 1953 value is above the indifference zone, the return to normal peace relations not being as clearly indicated as in the German scores. Table 2 shows that, of these four high scores, those for 1947 and 1950 have significant differences when compared with that for 1941, that for 1945 is almost significantly different from that for 1941, and the 1943 score lacks something of significance, although the ratio is not negligible.

Table 1. *Means of attitude scores and standard deviations*

	1940	1941	1942	1943	1944	1945	1946	1947	1950	1953
n	25	20	56	20	22	31	62	71	29	33
					Germans					
A.M.	5·4	5·3	5·1	5·3	5·2	5·7	5·7	5·6	5·3	5·1
S.D.	0·78	1·00	0·74	0·79	0·81	1·41	1·11	1·05	1·21	0·78
P.E.M.	0·1054	0·1509	0·0667	0·1192	0·1165	0·1708	0·0950	0·0840	0·1515	0·0900
					Japanese					
A.M.	5·1	4·9	5·2	5·5	5·3	5·5	5·3	5·5	5·5	5·3
S.D.	0·75	0·35	0·72	1·05	0·56	1·10	0·76	0·71	0·90	0·80
P.E.M.	0·1006	0·0524	0·0649	0·1584	0·0805	0·1333	0·0651	0·0568	0·1127	0·0900
					Maoris					
A.M.	4·8	4·1	4·8	5·3	5·2	5·5	5·0	5·3	5·0	4·8
S.D.	0·81	0·71	0·88	1·20	1·17	0·86	1·04	1·13	1·08	1·01
P.E.M.	0·1090	0·1065	0·0793	0·1810	0·1678	0·1059	0·0891	0·0900	0·1353	0·1185

We have noted a more decisive trend for the German attitude scores as against the Japanese ones to return to peace levels after the war. It must be remembered that Japan is a fairly near Pacific neighbour for New Zealanders and that a war situation comes more easily to mind with them than with the Germans. Near neighbours know a good deal about each other.

Table 2. *Significance of differences in attitude scores*

	Germans			Japanese			
	1942 and 1945	1942 and 1946	1942 and 1947	1941 and 1943	1941 and 1945	1941 and 1947	1941 and 1950
Diff.	0·6	0·6	0·5	0·6	0·6	0·6	0·6
P.E.D.	0·1833	0·1160	0·1072	0·1668	0·1432	0·0772	0·1242

	Maoris								
	1941 and 1940	1941 and 1942	1941 and 1943	1941 and 1944	1941 and 1945	1941 and 1946	1941 and 1947	1941 and 1950	1941 and 1953
Diff.	0·7	0·7	1·2	1·1	1·4	0·9	1·2	0·9	0·7
P.E.D.	0·1523	0·1328	0·2100	0·1987	0·1501	0·1388	0·1394	0·1721	0·1593

Whereas the changes in attitude toward Germans and Japanese were very much as expected, those toward the Maoris, New Zealand's early inhabitants, were little short of a complete surprise to the writer. There is certainly not the expected picture of a 'control' group. The Maori measures show the same kind of fluctuation as the Japanese and German, but at a lower (more favourable) level. The attitude scores of four years (1943, 1944, 1945 and 1947) are unfavourable, the others being favourable.

Two factors appear to have been at work here influencing the attitude toward Maoris. One, which has been noted frequently by workers in the social sciences, is a tendency in attitude toward one alien racial group to manifest itself similarly to other alien or somewhat alien groups. Although the Maoris are not markedly alien to New Zealanders, this movement has probably been effective. The second factor is related to a temporary maladjustment consequent upon large-scale movements of the Maoris from country to city during the war to meet urgent labour needs. Beaglehole (1955) supplies some figures on this problem.

When one studies the variability of the attitude scores one notices how closely the measures correlate with the attitude scores themselves. The standard deviation values when correlated with the means of the attitude scores, give a coefficient of 0·73 with the German values, 0·67 with the Japanese and 0·63 with the Maori values. In general, therefore, as the attitude scores increase or as the attitudes become less favourable, they become more variable or more heterogeneous. As they become more favourable they become more homogeneous or the situation becomes simpler. The variation for the three racial groups tended to increase with the later years of the war period and for some time after. By 1953 the attitude toward Germans was about as simple or homogeneous as at the beginning of the war, that toward Japanese had not moved completely back and that toward Maoris remained still relatively complex.

III. Comparison with a study by Remmers & Wood

Remmers & Wood (1947) had been conducting a similar study at about the same time in the United States. Using similar attitude scales they assessed the attitudes toward Germans, Nazis, Japanese and Jews of elementary psychology students at Purdue University. The years of testing were 1935, 1942, 1945 and 1947. The separation of Germans and Nazis is a real one in the United States which has a large German population. Such a contrast could not be well made in New Zealand by a student population.

The attitudes toward Germans were favourable throughout and most favourable in 1947, whereas those toward the Nazis became more and more unfavourable. The attitude toward the Japanese showed a similar trend to that toward the Nazis. By contrast there was no marked change in attitude toward the Jews, so they offered something of the benefit of a control group. The complexity in attitude to this group showed up in high variability throughout, with no improvement in 1947.

Considering the different racial relationships of a New Zealand student group as compared with an American one, the general findings of these two attitude studies are similar.

IV. Summary

1. Tests of racial attitudes using the Thurstone technique with similar samples of students at Auckland University College were carried out with reference to Germans, Japanese and Maoris. The testing was done each year from 1940 to 1947, then in 1950 and in 1953.

2. Changes in attitude scores and in variability such as might be expected during war and succeeding peace were found with reference to Germans and Japanese. The unfavourable attitude toward the Japanese remained, with high variability, even in 1953. The

attitude toward Germans had returned to indifference by 1953, with lowered variability. The attitude toward Maoris changed similarly but at a more favourable level.

3. The Thurstone technique as used here appears to be a sensitive instrument to detect major changes in racial attitude.

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NOTES ON THE SHAW TEST

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These notes are intended to publicize the Shaw Test and to suggest the setting up of a Research Exchange through which research work on the test may be facilitated. Full details, photographs, test instructions, etc., can be obtained from the author, and a copy of the test can be borrowed for a short period.

The Shaw Test was first used by Howson (1948). It consists of four wooden blocks which can be arranged in fifteen different series: according to height, weight, colour, position of notch, etc. In addition to these fifteen normal, abstract and clearly defined sequences, which are called Grade 'A' responses, there are at least forty-one less frequent or less clearly defined sequences which are called Grade 'B' responses, e.g. the volume of the blocks, the positional relation between the word and the notch, etc., which are positively but not significantly associated with Grade 'A' responses. There are at least sixteen Grade 'C' responses, mostly illogical, confused and concrete, e.g. making patterns with the blocks, grouping the blocks instead of serializing them, etc., which are negatively but not significantly associated with Grade 'A' responses. Finally, there are at least five Grade 'D' responses, the poorest, mostly failures of comprehension and inane responses, e.g. building towers with the blocks, simply describing the blocks, etc., which are negatively and significantly associated with Grade 'A' responses.

This classification of responses was made through an objective item-analysis of the test performances of 256 relatively normal adults ranging in age from 17 to 82 years. No time limit was imposed and the subjects were, in a sense, 'tested to the limits' of their capacity for producing ideas. The general significance of the responses is that the good and fairly good (A and B) ones indicate the speed, power, originality and general efficiency of the subject's thinking, whilst the fairly poor and poor (C and D) ones seem to indicate the abnormality, defectiveness or degree of deterioration of the subject's thinking. A reliability coefficient for the Shaw Test Grade 'A' responses, computed by the method of rational equivalence, came to 0·87. The correlation between Grade 'A' responses and the Wechsler-Bellevue Efficiency Quotient was +0·745 (+0·641 with age held constant). The correlation between Grade 'D' responses and WBEQ was −0·505 (−0·303 with age held constant).

Other results suggest that the Shaw Test is a promising research instrument for psychometrics and psychodiagnostics. Howson found the test useful in disclosing the fact of but not the degree or location of impairment in brain-injury cases, but there is reason to believe that he did not make full use of the test's potentialities. The safest judgement that can be made at the moment is that the test is a fairly reliable measure of *fluid intellectual ability*. It is one of the few if not the only performance test involving the mental operation of *serializing*, and this fact alone makes the test interesting. If it is used as a timed test on superior subjects, then it almost certainly involves some kind of *perceptual fluency*, and may be of help in defining those factors which refer to high-grade thinking abilities (cf. Guilford, Kettner, and Christensen, 1954). Experimental investigation shows that

Shaw Test performance is influenced by age in exactly the same ways as *creative intellectual achievement* (cf. Lehman, 1953). The test can be adapted as a measure of *originality or conformity*; Klopfer & Kelley (1946) describe a similar classification of responses and give them a significance that could well be applied to Shaw Test responses: popular (frequently occurring) good responses indicate a degree of normal activity and conformity, whereas original (infrequently occurring) good responses indicate an above average degree of activity and brilliance or creativity. It is now commonly understood that tests may not measure the same processes in different subjects or under different conditions, and the Shaw Test is probably no exception.

The fact that Shaw Test performance can be analysed qualitatively and quantitatively in an objective manner, plus the fact that C and D responses are symptomatic of a deteriorated intellect, suggest that the Shaw Test may be useful diagnostically, since abstraction tests are among the more sensitive of instruments for detecting disordered and defective thought processes, and any promising addition to current test-batteries deserves careful consideration (cf. Yates, 1954). An examination of the poor quality (C and D) responses obtained from normal old people suggests that normal intellectual decline approximates to a condition not unlike that found in mental defect and brain injury. A close examination of the number and quality of Shaw Test responses may reward the clinical research worker by helping him to answer questions about and make differential diagnoses in the various psychiatric and neurological disorders, since C and D responses are *signs of impairment*. A Shaw Test Deterioration Quotient can be calculated by taking: $C+D \times 100/A+B+C+D$; this D.Q. has some advantages over that used by Wechsler (1944), but needs further examination.

The main advantages of the Shaw Test are that it is a relatively novel psychological test and adaptable to a variety of uses. It is compact and easy to construct, convenient and quick to administer; the scoring is objective and the task is interesting to the subject. The test's disadvantages are that no norms are yet available, other than those compiled by this author on 256 normal and superior adults, and validity studies are incomplete—but this is unavoidable with new research tools.

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PUBLICATIONS RECENTLY RECEIVED

Psychology as the Study of Behaviour. An Inaugural Lecture by O. L. Zangwill, Professor of Experimental Psychology in the University of Cambridge. Cambridge University Press. 1954. Pp. 36. 2*s.* 6*d.* net.

The appointment of the second professor of Psychology at Cambridge, in succession to Sir Frederic Bartlett, is an event of exceptional interest and importance. As two points are needed to define a line, so presumably a succession of professors is required either to establish a tradition or to define a direction of change.

Cambridge, one might have supposed, was destined from eternity to be the home of a very 'pure' psychology and perhaps of a very distinctive 'school'. In fact it has been the home of neither. The history of its psychological laboratory reflects the influence of two world wars and its most characteristic contributions to science have been in the study of very practical skills. Under the statesmanly direction of Sir Frederic Bartlett it has established a reputation for applied psychology conducted in the spirit of fundamental research. So far from forming a 'school' those who have passed through Cambridge to chairs of psychology and other positions of responsibility are characterized by the diversity of their interests, convictions and talents. Perhaps the only thing they have in common is a comparative immunity from what has been an occupational disease of psychologists, addiction to the membership of schools.

And now we ask: Whither psychology at Cambridge?

This inaugural lecture (delivered on 12 November 1954) is both a review of the history of psychology at Cambridge, and a statement of its new professor's view as to what psychology is and what it should be. The two themes are interwoven since Professor Zangwill, though not overmuch concerned with the applications of psychology, is very much concerned in other ways to preserve and develop the approach of those who have made psychology at Cambridge what it is to-day.

Psychology, no doubt, is what psychologists actually do, and in the end there may be little more that can be said; but the fact that what psychologists actually do in some places is to be amateur philosophers, and at others to be amateur neurologists, naturally engenders a desire for a definition a little less ostensive. Psychology, in this lecture, is defined as the Study of Behaviour. But let no one be mistaken: the Study of Behaviour is not Behaviourism in any of its varieties catalogued up to date. Professor Zangwill picks his way with the utmost circumspection, avoiding the many snares in the path of those who would so describe their subject. He begins by paying due tribute to McDougall, the first to define Psychology in this way, but he shares with other naturalistic psychologists a certain distrust of McDougall's concession to tradition. Whilst deploring Watson's 'brash pronouncements on the non-existence of mind', and regretfully doubting whether advances in the study of animal behaviour have led to corresponding advances in the understanding of human behaviour, he believes, nevertheless, that the attitude of mind which Behaviourism reflected has made possible empirical advances on a broad front. He regards the rapprochement between psychology and physiology as the most important outcome of the Behaviourist movement. If it is for this reason that he gives Watson and his associates very high marks, it is for the same reason that he gives very low marks to the Neo-Behaviourists who in their desire to emancipate psychology not only from philosophy but also from neurology and so to establish a science of 'pure behaviour' have replaced the brain by a tangle of hypothetical constructs and intervening variables.

Professor Zangwill's most positive statement of his own position is framed with characteristic caution: 'I do not regard a human psychology based on consideration of nervous structure and function as wholly without the bounds of possibility.'

How far his own position is removed from doctrinaire Behaviourism is clear when he 'ventures to predict' that at no time will even the sensory physiologist (still less presumably the psychologist) be able wholly to dispense with the testimony of the human observer. If there were a doubt concerning the breadth and tolerance of Professor Zangwill's approach to his subject it would not survive the final statement of his *credo*. This is implicit in a warm appreciation of W. H. R. Rivers: 'I do not believe that we can go far wrong if we model our conception of psychology on the interests and outlook of its first accredited teacher at Cambridge.' This, of course, lets in everything. Rivers was a natural scientist with

an exceptionally flexible mind. He could be physiologist, anthropologist, introspectionist or psychoanalyst in his methodology as the problem seemed to require. Where the memory of Rivers is preserved, it would certainly be hard for things to go far wrong. The Department of Psychology at Cambridge is in safe keeping.

C. A. MACE

Experimental Psychology. By B. A. Farrell (editor) and others. (A Series of Broadcast Talks on Recent Research.) Oxford: Basil Blackwell. 1955. Pp. xi + 66. 7*s.* 6*d.*

This short volume places on record six broadcast talks given by different speakers in the Third Programme of the B.B.C. in the summer of 1954. The common bond between the speakers lies in their connexion with the development of psychology in the Universities of Oxford, Cambridge and Reading. The individual topics belong to a very wide conception of experimental psychology. The sample covered included: I. Perception (Watson); II. Adult Learning and Remembering (Kay); III. Motivation (Deutsch); IV. Some Hypotheses in Psychoanalysis (Farrell); V. The Study of Social Behaviour (Argyle); VI. The Prospects of Experimental Psychology (Oldfield). Each contributor dealt with a topic of special personal interest.

As the editor correctly indicates, a different group of speakers would have chosen a different set of topics. Furthermore, no claim is made that these talks are to be considered as contributions to psychology. They are directed towards the intelligent and educated layman in an attempt to remove certain current 'misconceptions which surround the work of the experimental psychologist'. From that standpoint the book is of considerable interest to all who are concerned with the 'public relations' of contemporary psychology. The 'recent research' referred to covers roughly the last fifteen years. Only in one talk (II) is there substantial emphasis on work carried out by the particular contributor. In most cases the talks are topical reviews in a conversational style of important literature. In spite of an excellent final talk by Prof. Oldfield giving a perspective to the whole series one wonders whether the layman will not feel that the contributions of experimental psychology are rather trivial. Is such a distortion an inherent limitation of a short exposition on a complex series of topics? Does the popularization of psychology demand some form of anecdotal treatment of a type from which academic psychology has only recently emancipated itself? Such questions cannot be examined here.

From the point of view of the serious student of psychology this book might well be recommended as background reading. Its broad perspective should be valuable to the student in the early stages of courses in experimental psychology when motivation tends to decline owing to the necessarily segmental and artificial nature of elementary experimental work.

ROLAND HARPER

Vision with Spatial Inversion. By F. W. Snyder and N. H. Pronko. Wichita, U.S.A.: University of Wichita Press. 1952. Pp. 144. No price given.

The authors describe an experiment in which the subject wore inverting spectacles for 30 days, covering the eyes with a bandage whenever the spectacles were removed. The spectacles were in the form of 'unit-power' inverting telescopes, attached by appropriate head-gear. They gave a clear binocular field of 20 degrees diameter, and were set in a slightly convergent position in order to permit optimum binocular vision for near objects. The subject carried out systematic practice without the inverting lenses on the following tasks in the 'pre-inversion' period: card sorting, Purdue Pegboard, Minnesota Rate of Multiplication Test, mirror tracing. Then he continued the learning tasks with the inverting spectacles in place, for 30 days, and finally completed the practice at the tasks after the spectacles were permanently removed. Full details of the learning experiments are given. The narrowing of the binocular field to 20 degrees alone did not interfere to any extent with the tasks, but when the lenses were first put on the visuo-motor co-ordinations were seriously disrupted. Later the subject gradually adapted himself to the inverted vision and improved at the learning tasks even more than before the lenses were worn. Removal of the lenses again disrupted the visuo-motor co-ordinations for a short time. The authors infer that space perception does not exist at birth, but comes into being as a result of a series of 'contacts' between the organism and the environment. The subject's own observations make it abundantly clear that the question whether there was a true phenomenal reversion of the inverted image to uprightness would have been a false problem. At first he saw objects upside-down and inverted from side-to-side. Later, when visuo-motor co-ordinations were fully established he was able to read, sketch, to walk about and even to drive a car, and it did not occur to him to doubt the uprightness of his visual pictures. When asked whether

things looked upside-down, he replied, 'I wish you hadn't asked me. Things were all right until you popped the question at me. Now, when I recall how they *did* look *before* I put on these lenses, I must answer that they do look upside-down *now*. But until the moment that you asked me I was absolutely unaware of it.' What he saw had appeared quite natural until his companion asked him, just as much so as in his pre-inversion days. When a clown was seen to jump off the diving board into a swimming pool he appeared to move downward into the pool, but in fact the way he seemed to move depended upon whether or not the subject made reference to his memory of the pre-inversion way of seeing things.

R. W. PICKFORD

Destiny and Motivation in Language. By A. A. Roback. Cambridge, Mass.: Sci-Art Publishers. 1954. Pp. 474. $8.50.

The subtitle of this volume is *Studies in Psycholinguistics and Glossodynamics*. In a copious Glossary which is—fortunately—provided 'Psycholinguistics' is defined as 'The phase of linguistics which introduces psychological considerations'; and 'Glossodynamics' as 'The psychological phase of linguistics which deals particularly with motivation either on the sound level, meaning level, or grammar level'. It is to be doubted whether these definitions make anything very much more clear. Psychological considerations seem as if they can be introduced into *any* phase of linguistics, while the motivation that is referred to might be whatever active human processes can be supposed to give rise to particular sounds, meanings or grammatical forms and their derivations over a long period of time; or it might be whatever activities particular sounds, meanings or grammatical forms themselves set up, or it might be both of these, or, apparently, it might be neither of these, but something else. The fact is that this is a book into which Dr Roback can introduce anything whatever that he wants to say about language. In the result the volume is an excessively difficult one to read straight through, for it turns and twists about all over the place according to what may perhaps be called the very lively motivation of its author. But the psychologically minded reader who dips, and then comes to the surface and ponders a while, and later dips again, will not only get a lot of enjoyment out of the exercise, but will also probably be started upon many interesting and profitable explorations.

Part I of the book deals with 'historical symbolism', and this is where most of the valuable and unconventional discussion about 'destiny' is to be found. Part II is headed 'Glossodynamics', and, broadly, is concerned with psychological forces that may have led to language, to special languages, and to the interplay of languages. Part III is called 'ethnolinguistics', and a good many readers are likely to find this the most attractive and original part of the whole treatment. It contains many significant and often brilliant comments upon language forms and constructions that have become particularly frequent or favoured in various social and cultural groups.

Nobody could reasonably call *Destiny and Motivation in Language* an easy work to read. Its range of learning is prodigious, its style of writing is often a mixture of the severe and proper with the airily conversational, and the direction of the argument seems sometimes to get rather lost among ramifications of detail. But it is a book which nobody who is, or wants to be, interested in the psychology of communication can afford to neglect, though it has no truck whatsoever with Cybernetics or Information Theory. Once more, Dr Roback has shown that it is extremely difficult for him to write at length about anything without being profoundly interesting.

F. C. BARTLETT

Child Psychology. (Fourth edition.) By Arthur T. Jersild. London: Staples Press. 1955. Pp. xii+676. 42*s*.

This most recent edition of a well-known text on Child Psychology is described as 'virtually a new book'. The previous editions have been brought up to date by the inclusion of recent material, but the basic plan and approach is still recognizably that of the earlier versions. The emphasis is still on the total growth of the child, with particular attention being given to the emotions and personality. There is no doubt that the revision has been an improvement; the particular virtue of the latest edition being not so much in the inclusion of recent studies, as in the deeper and more mature treatment of children's development. There is much wisdom and humanity in it, but not at the expense of science. The excellent bibliography deserves special mention.

Those who are familiar with the earlier versions will perhaps not find as great a change as is claimed; to those who have not made their acquaintance with this book, it can be highly recommended.

JAMES MAXWELL

The Study of Personality: a Book of Readings. By HOWARD BRAND (ed.). New York: Wiley and Sons; London: Chapman and Hall. 1954. Pp. xvi+581. 48*s*.

This book attempts a cross-sectional view of research on personality. The editor has written introductory passages and there are thirty-one contributions, principally from journal articles. Contributions in the 'Theory' section include articles by Alexander, Allport, Eysenck and Sullivan; in the 'Methods' section are Stephenson on Q-technique, Thouless on psychopathology, and others; finally in 'Problems' Newcomb writes on role behaviour, Hilgard and Maslow on motivation, while Sears, Frenkel-Brunwik and others are also represented. Yet despite these and other distinguished contributors the book is a disappointing one; its weaknesses are of editorship in that the articles have been badly chosen. To illustrate, from G. W. Allport's large bibliography two papers are published. The first comprises Allport's unsympathetic assessment of Stern who, one feels, might well have been permitted to provide his own exposition; the second article consists largely of Allport's comments on Mr Bertocci's opinion of Allport's earlier comments on McDougall! For a book of this kind many of the other papers included would seem equally difficult to justify. Again, some of the strongest parts of the book—N. E. Miller on displacement and Eysenck on the literature of schizothymia—are readily available in quite recent numbers of the principal APA or other well-known journals.

As a whole this book does not achieve its stated aim, and moreover certain individual contributions merit rigorous criticisms in terms of their scientific worth. Thus C. S. Hall deals with dream analysis from a standpoint which adds nothing important to the theory of Rivers, and proceeds without either evidence of awareness of, or influence by, this earlier and better worked-out formulation. Again, Farrell's chapter on the scientific testing of psychoanalytic work proceeds with the apparent assumption that such 'testing' can be achieved largely by citing the names and dates of relevant investigators. The argument thus comes to depend more on the prestige of names than on any attempt at evaluation of the scientific quality of the investigations themselves: surely a poor lesson in scientific methodology. Finally, a major defect is that two of the most vigorous and important developments in personality study—Learning Theory and the Functional Perception movement—are both badly under-represented.

PETER MCKELLAR

The Interpretation of Dreams. By SIGMUND FREUD. Translated and edited by JAMES STRACHEY. London: Allen and Unwin. 1955. Pp. xxxii+692. 21*s*. net.

As the translator points out, Freud regarded *Die Traumdeutung* as his most important work. 'Insight such as this', he wrote, 'falls to man's lot but once in a life-time.' The edition here reviewed is practically identical with that contained in vols. IV and V of the standard edition. Since these volumes are not obtainable separately from the set, this edition is assured of its welcome. In addition to being an entirely new translation, it is a Variorum edition, with full indications of the stages at which Freud introduced his many alterations in successive versions of the text; the supplementary material—bibliographies and indices—has been greatly expanded and completely recast.

Strachey's translation is perhaps freer than Brill's, and his style probably more remote from Freud's own, but greater clarity has usually been achieved. Most compound German nouns (of which *Traumdeutung* itself may be regarded as a prototype), customarily represented in translation by hyphenated equivalents, are now rendered into more idiomatic English usage. Many other changes in minor but familiar detail are noticeable: e.g. *secondary elaboration*—regarded as a mistranslation—is replaced by *secondary revision*. On the other hand, the word play so characteristic of the dreams recounted by Freud is dealt with very literally, even pedantically, a good fault—if it is a fault at all—in a work designed to be scientific rather than entertaining.

The book is admirably produced, and most inexpensive by present-day standards.

BORIS SEMEONOFF

The Mythology of the Soul. By H. G. BAYNES. London: Routledge and Kegan Paul, 1955. Pp. xiii+939. 50*s*. net.

The re-issue of this important book will be acclaimed by analytical psychologists and psychiatrists. C. G. Jung's monumental contribution to the modern methods of psychotherapy and his immeasurable influence on Western thought and feeling unceasingly call for new evaluations and elaborations of case

material by those analysts who—like Baynes—are using the concepts of the Collective Unconscious (i.e. the impersonal unconscious of mankind, the race, the nation, the family) in their work and thought. As at first most of the analytical literature naturally stemmed from Middle European sources, it was fitting that Baynes should be one of the first contributors in Britain. Himself a pupil and friend of Jung, he remained for many years until his premature death in 1944 the inspired leader of the Jungian School in England, and his original and congenial personality has left its distinct mark on Analytical Psychology as it has grown from its own collective roots in these islands.

Baynes was, indeed, undeniably British. A foreign admirer once whispered at the onset of one of his lectures: 'This man is surely not a psychologist: he is an English Gentleman.' The reader of his *Mythology of the Soul*, however, will be equally impressed by the depth of his psychological knowledge and his constructive intuition as by the width of his cultural background and the sureness of touch in his investigation.

The task which he sets himself in this scholarly work is the detailed evaluation of analytical material of two schizophrenic patients. Schizophrenia is perhaps *the* mental illness most typical of man's inherent psychic condition. Religious and philosophical systems have recognized the existing split since the beginning of time, and while the former have allowed mythological expression of it, they both have sought to devise formulae for the solution of the inner conflict in mankind's divided soul. Baynes has no formula to offer and while he uses the relevant mythology to the fullest possible extent, he endeavours to reach down to the very roots of the catastrophic split which we all have to accept as our own to some degree in order to kindle the therapeutic light in the darkness within and thus begin to heal the great ill. He uses the individual material to elucidate man's fundamental problem, and his readers can hardly fail to follow his inspiring leadership into their own depth.

The book is full of valuable and most original teaching. One may at times feel inclined to separate some of his revealing formulations and descriptions of psychological conditions from the drama of the individual dreams and drawings, because the latter is naturally far less accessible in all its microscopic detail to the reader. He must lack the transference link which exists between the analyst and his patient, in spite of Baynes's warm-hearted and capable handling. The derived formulations, on the other hand, which are at times threatened to be drowned by the enormous power of the analytical drama, could well fill a handsome volume of psychological aphorisms of great and lasting value.

The book is beautifully produced, and the publishers must be congratulated on the skilful reproduction of the many drawings. To facilitate the study of the invaluable material, it would still be better if the drawings could be on separate sheets loosely attached to the volume so that the reader could compare text and material at greater ease.

W. P. KRAEMER

The Many Faces of Love. By Hubert Benoit. London: Routledge and Kegan Paul. 1955. Pp. vii + 308. 28*s*. net.

This book, translated from the French by P. Mairet, is written by an ex-surgeon who, after being injured in the War, turned to the study of psychology. Dr Benoit has clearly a most sensitive appreciation of human nature coupled with a love of philosophy—indeed his work might appeal more to the philosopher than to the scientist. He considers that there are three basic aspects of love—adoration, benevolent and appetitive love, and he investigates the ways in which they fuse and interact. He also discusses various perversions and inhibitions of normal love, but although he has implicitly taken over some of the concepts of psychoanalysis, his thoughts will sound foreign to those who believe in the importance of the Oedipus complex, castration anxiety, and early object-relations in the development of the ability to love.

The work itself is written in the form of a dialogue between the author, a young man and a young woman. In spite of this literary form, it is extremely difficult to read. Perhaps this is best illustrated by a quotation:

'*The Author*:...In the last stage of man's realization, the realization that is timeless, the triangle is constituted otherwise: the passive, feminine pole is represented by the totality of the temporal aspect of the man, both his "soma" and his "psyche"; the masculine, creative pole is the Creative Principle which is at the centre of every created being, the nucleus we sometimes call "Spirit", in the sense in which that "Spirit" is prior to both the "body" and the "soul". The "Spirit" penetrates the psychosomatic organism of man, in an ultimate interior "coitus", by virtue of a hypostasis which is then the very principle of Love, for which the scholastic term is "Agape", or the active love of the Supreme Principle

for his passive creature. The result of that act of love is, then, the "new man", man "liberated", "realized", reunited to the Principle of the whole of manifest being.'

'*The Young Man*: I confess, sir, that these views bewilder me.'

The reviewer agrees.

JOSEPH SANDLER

Social Psychiatry. By MAXWELL JONES. London: Tavistock Publications Ltd. 1952. Pp. xix+186. 18*s*. net.

'The development of community methods of treatment implies the wider application of the social sciences, mainly sociology and anthropology to existing psychiatric practice', writes Dr Jones in his introduction. Along with chapters written by his colleagues he reports on some five years' work with a therapeutic community, the Industrial Neurosis Unit at Belmont Hospital, Sutton, Surrey. Two early experimental communities, the Effort Syndrome Unit at Mill Hill Emergency Hospital, and the Ex-Prisoner-of-War Unit at the Southern General Hospital, Dartford, are described. By May 1945, when the latter unit was begun, three concepts were emerging: treatment was a continuous process operating throughout the entire waking life of the patient in hospital: a re-organization of the hospital society was needed with a greater degree of social penetration between patients, nurses and doctors: the patient's role in the hospital society needed scrutiny.

The third community, with which the remaining chapters are concerned, was established in April 1947 at Belmont Hospital with the backing of the Ministries of Health, Labour and Pensions. Its main function was to study the chronic unemployed neurotic. The population of the unit (100) comprises in the main chronic neurotics and character disorders of a kind usually considered unsuited for treatment by psychotherapy or physical methods.

Dr Jones defines a role as a set of legitimate expectations of behaviour and then proceeds to outline the social structure of the industrial neurosis unit with nurses (authoritative social therapeutic roles), doctors (social supportive exemplary activating and interpretative), disablement resettlement officers, social workers, psychologist and patients.

The three following chapters by Dr Jones, by Dr Thomas Freeman and by Dr B. A. Pomryn offer a more detailed account under the heading of social therapies, in-patient psychotherapy and techniques of group formation. The viewpoint here is non-specificist. Neurosis is not an illness—'by neurotic illness we mean emotional difficulties, conscious or unconscious, which result in an inability on the part of the individual to meet the needs of his environment, or to make adjustments (at a conscious or unconscious level) sufficient to maintain an adequate functional efficiency'. How such a group of persons, staff and patients, function therapeutically is illustrated by an account of the unit discussion groups and psychodrama as part of the subculture of the unit.

Six months after the first industrial case was discharged from hospital, a follow-up study was begun. Two groups were investigated: (*a*) patients who had attended the industrial neurosis unit, and (*b*) disabled neurotic unemployed people, who had not attended a rehabilitation treatment centre such as the Unit, and who were referred by the Employment Exchanges of the Ministry of Labour. 104 of 238 consecutive discharges were included. Joy Tuxford outlines the follow-up interview and the general aspects. In the course of this chapter there are several sociological impressions for which one hopes someone else will provide evidence—'there was in most areas a real paucity of group life', or 'the family, which was in most instances a small and loosely knit group, was often the only one which had any real meaning for the individual'.

Joseph Sandler writes on the statistical analysis of the follow up and on the concept of general adjustment. An adjustment score was calculated for each patient and related to the information recorded in hospital. 'The fact that a number of antecedent measures are significantly related to adjustment means that it is possible to predict which patients will do well and which badly, to a degree of accuracy better than might be expected by chance alone.' Unit doctor's diagnosis, doctor's assessments of improvement in hospital and marriage, for example, are associated significantly with good adjustment. Six months after leaving hospital two-thirds of patients followed up had made a fair adjustment or better: just over one-half had worked full-time since hospital.

Doctor Sandler's chapter on Vocational Guidance contains some very important comments and is recommended not only to the vocational guidance psychologist but to the clinical, educational and industrial psychologist.

Dr Jones writes a final chapter. 'Our findings appear to justify the conclusion that it is possible to change social attitudes in relatively de-socialised patients with severe character disorders, provided they are treated together in a therapeutic community.' He goes on to talk of the hospital in the light of some of the concepts of social science which will be familiar to most readers of this *Journal* but are likely to be rather revolutionary to some medical men.

There is some compensation for the lateness of this review in being able to recommend also the article by Baker, Jones, Pomryn and Merry, 'A Community Method of Psychotherapy' in the *British Journal of Medical Psychology*, XXVI, 222–44, 1953, which is, as the authors say, 'complementary to our recently published book'.

For some readers the term Social Psychiatry is likely to excite as much emotion as Social Medicine, but names are not all-important. This book is recommended to both doctor and psychologist. It overstresses neither heredity nor environment: it sees behaviour as a resultant, and modifiable. It is a modest account of an exciting experiment.

JOHN C. KENNA

Einführung in die medizinische Psychologie. By GEORG DESTUNIS. Berlin: Walter de Gruyter and Co. 1955. Pp. xi+218. DM. 22.

Half of this book is devoted to the normal and the other half to the neurotic personality. 'Personality' is defined exlusively in terms of philosophical psychology, and formulations of the Leipzig *Ganzheitsschule* (Krüger-Sander-Wellek) are mainly preferred. Similarly, intuitive assumptions on the nature of traits, their integration and disintegration, are accepted throughout in preference to operational definitions.

Normal personality, in the author's view, is made up of fourteen drives of maintenance, sex, aggression, curiosity, etc. In addition, consideration is given to motivation, sentiment, affection and emotion and, finally, the six noetic functions, will, perception, imagery, memory, thinking and intelligence. A remark directed against the definition of intelligence by Spearman and others that 'these concepts were only considering the elements (i.e. neglecting the whole)' expresses the author's desire to decline any factual analysis.

In the following chapter on psychodiagnostics a few known tests of intelligence, child development, association and projection are briefly and uncritically reported. A description of the historical continental typologies concludes the first part.

Part II, on neurosis, presents in an elementary fashion a comprehensive variety of present-day German theories, and then deals with conventional descriptions of neurotic psychogenic, psychosomatic and psycho-organic symptomatology. A short chapter on neurotic-like reactions during illness is added.

The bibliography consists of almost 250 items, roughly 85% in German, 10% in English and 5% in French. Half of these date back 20 years or more. Considering the wide range of topics covered in this book, this selection of references suggests that the author's attempt 'to summarize and to evaluate the most recent results in the field of medical psychology' can hardly be considered to have been achieved.

J. C. BRENGELMANN

La notion de normal en Psychologie clinique. By FR. DUYCKAERTS. Paris: Libraire Philosophique J. Vrin. 1954. Pp. 206. No price given.

This book, published in a series called 'Problèmes et Controverses', is a philosophical discussion of the psychological notion of normal behaviour.

The author opposes those who contend that normality can only be a relativistic notion, either because they fear the contamination of psychological science by ethical judgements or because they are struck by the differences in accepted behaviour of separate social groups. He attempts to build up an objective definition of normality and the use of psychotherapy is then for him *ipso facto* justified.

The author chooses to discuss normality first as 'integration', then as 'autonomy' in relation to other individuals, as 'adaptation' to different social groups, and finally as resemblance to the average. The consideration of normal behaviour as 'integrated' behaviour is by far the most interesting and stimulating chapter. The final definition of 'integrated' behaviour is related to observations made by such authors as Schwarz, Goldstein and Maier. The common thesis of these authors is to distinguish between the significant, ordered, motivated forms of behaviour on the one hand, and the catastrophic or frustrated forms of behaviour on the other, and on this distinction in the main the author bases his definition: to be normal, behaviour must be purposeful.

Autonomy and adaptation are perceived as complementary to each other, and from an analysis of their meaning, the author derives an added necessary qualification of his definition of normal behaviour; it must be creative, in other words, it must include an assimilation of the past and of surrounding cultural values and aim at the building up of effective social contact.

The concept of normality identified with statistical average is rejected with horror. Kant is called in to help in the forty-page demonstration that such heresy does not fulfil any of the criteria of a logical definition, and in a mood of angry humour the author attempts to unveil the motives of those whom he believes to uphold such a notion.

The book will disconcert the reader who is more used to Anglo-Saxon literature. It is so frankly and unashamedly theoretical and philosophical, so devoid of any empiricism, so direct in utilizing facts as examples illustrating the author's ideas—instead of building those ideas on a succession of facts.

In the tradition of French contributions to philosophy and to science, the weightiest part of the book is the critical discussion and clarification of ambiguous notions, but when the author comes to the construction of practical concepts, he is guilty of certain evasiveness. The norm which the author finally defines is both absolute and yet infinitely varied in its mode of expression, but although it thus satisfies not only logic but also elegance of thought, it defies any attempt to make it serve as an operational and useful criterion.

MARYSE METCALFE

Psychoanalytic Interpretation in Rorschach Testing. By Roy Schafer, Ph.D. Pp. 446. New York: Grune and Stratton. $8.75.

In 1923, with the posthumous publication of the case-study entitled 'The Application of the Interpretation of Form to Psychoanalysis', the marriage of the Rorschach method with psychoanalysis was announced by its founder. Dr Roy Schafer cements this fruitful alliance in his valuable book, which to a notable extent co-ordinates and enlarges the work of other writers in the field, adding much that is original with a refreshing clarity of reasoning and economy of words. As is stated in the preface, this is not an easy book to read, for to profit by it adequately there is required not only a thorough grounding in Rorschach principles and technique but also considerable familiarity with psychoanalytic literature and methods. This warning, however, should not deter any serious Rorschach worker, since at every point hypotheses are clearly stated before any conclusion is drawn, so that the reader is gently educated as he goes.

Main themes discussed are:

(*a*) Interpersonal dynamics in the test situation.
(*b*) The response process.
(*c*) Thematic analysis.
(*d*) Criteria for judging the adequacy of interpretations.
(*e*) The interpretations of defence-mechanisms.

In recent years there has been an increasingly healthy recognition that the personality of the tester, and especially his unconscious needs, may seriously influence test results as well as their interpretation. The 'toilet of the analyst' is no less important for validity than is the intra-personal discipline of the clinical psychologist engaged in exploring the personality of others. Without such discipline he may lay undue stress on the voyeuristic aspect of his role, or, feeling anxious or guilty about this, may swing to an attitude described by Dr Schafer as that of a psychological ringmaster, his insistent directions arousing in the patient 'anxiety, inadequacy, compliance, rebellion, and a host of other reaction tendencies'. Other unconscious drives may cause the tester to stress the magically omniscient, autocratic, aspect of his role or the 'saintly', also with consequent effects upon the subject. Appropriately, this discussion of the dynamics within the tester is followed by an equally careful study of the dynamics of being tested. Dr Schafer never forgets the patient in a test situation and shows a rare understanding of the feelings which this may arouse.

In the chapter on 'The Response Process', some of the test instructions, based on those of Rapaport, might be challenged as over-directive or seductive, but the description of shifts in the level of psychic functioning, associated with Freud's concepts of 'primary process' and 'secondary process' thinking, is both valuable and illuminating. Both processes may be observed in most Rorschach records, for although 'on the advanced levels of psychic functioning the reality principle and the secondary process prevail over the pleasure principle and the primary process'...'the primitive levels do not go out of existence.... They frequently co-determine responses with the advanced levels, putting a personal stamp—possibly

too personal—on would-be impersonal, objective reactions.' In this way there is demonstrated the interplay of the forces of id, superego and ego which produce a picture of the man as he is with his child-self within him, integrated or unintegrated. The value for diagnosis of this assessment of dynamics is obvious, as is the later study of defences.

The chapter on thematic analysis, though partly schematic, includes an original contribution to the dynamics of content analysis, its uses and pitfalls, these latter being emphasized in the following chapter. All those using projective tests, especially those who tend to jump to conclusions when interpreting Rorschach material, will profit by studying Dr Schafer's criteria for judging the adequacy of interpretations. These provide a salutary check upon intuitive and get-there-quick methods with over-facile deductions.

The long section on the interpretation of defences, which comprises two-thirds of the book, is of outstanding value. Others with psychoanalytic orientation have taught that analysis of the mechanisms of ego-defence is essential for full understanding of Rorschach data, but here there is published for the first time a well-documented and expanded study of the subject with a wealth of illustration to clarify its points. From an admirable summary of the psychoanalytic concept of defence the reader is led to consider defensive operations in the Rorschach response process. In his introduction to this section and in the subsequent chapters the author uses all that he and the Rorschach method have got, and the result is impressive. 'Defensive operations' are related to scores, to images and attitudes, and to all combinations of these. Various mechanisms of defence are discussed in great detail as illustrated by psychogram and sequence analysis combined, demonstrating such mechanisms as repression, denial, projection, and the obsessive-compulsive. Although others might have been included, those quoted serve to demonstrate the function of unconscious defence in the healthily adjusted, as well as in the fields of neurosis and psychosis of varying types.

The level of this publication is so high that one is led to wish for expansion in some respects, as, for example, a more sensitive scoring of shading that would illuminate the tactile responses as associated with early feelings in the mother-child relationship. More about depression might also have been welcome, but it is unfair to ask for everything, and the reader can certainly give thanks for what he has received, with respectful congratulations to the writer.

THEODORA ALCOCK

The Adult-Child Interaction Test: a Projective Test for use in Research. By THERON ALEXANDER. University of Illinois, U.S.A. Monograph of the Society for Research in Child Development Inc., Vol. XVII, Serial no. 55, no. 2. 1952. Pp. v+40, with 8 plates. No price given.

'This test is planned to give information primarily about perceptive experience of adults in reference to children and children's perception of adults.' As such the test contains the germ of a valuable idea for a projective approach. But the method here developed is disappointing. The rationale offered neglects the more important questions concerning perception and personality, nor does it give any framework within which to think about motivations in terms of interpersonal relations. In the few pages devoted to the theoretical basis of the test we find vague generalizations which gloss over these important issues. 'Broadly speaking, human behaviour consists of the selection of stimuli for admission to the organism (controlled by the use of energy and reactivity of the receptors), the assignment of meaning of stimuli coming from the receptors, and selection of responses and the activities of the body system.... Finally, as a result of the organism's experience, characteristic individual approaches to problems and individual perceptions develop.'

With such a general basis and with only the most slender references to any clinical appraisal of behaviour, it is not surprising that the selection of picture situations shows no originality, that the method of analysis provides little of insight into personality dynamics and that the conclusions seem naïve and superficial.

H. PHILLIPSON

The Language of Dynamic Psychology as related to Motivation Research. By JOSEPH W. WULFLECK and EDWARD M. BENNETT. New York, Toronto and London: McGraw-Hill. 1954. Pp. 111. 30*s*.

Sponsored by the Advertising Research Foundation, this well-produced but expensive little book is intended to 'make the books, reports, and writings of dynamic psychologists and motivation research specialists of much more use to advertising men and other readers concerned with human behaviour'.

Taken at its own valuation it affords quite stimulating and at times even entertaining reading. About 600 terms, presented alphabetically, are defined, with illustrative comments. Many psychological and allied fields are represented—even some Rorschach symbols are included. There are, however, some surprising statements; e.g. of *statistical trend*: 'The phrase may be used in much the same way as the word *correlation*.' And under the heading *voyeur*: 'The desire to look at things which are taboo, concealed, and hidden, is trained into little boys at a tender age by their fathers.'

BORIS SEMEONOFF

Psychological Tests for Accident Proneness and Industrial Proficiency. By E. G. CHAMBERS. London: H.M. Stationery Office. Medical Research Council Memorandum no. 31. 1955. Pp. iii+30. 2*s*. net.

This Memorandum summarizes five I.H.R.B. reports by E. Farmer and the author, published between 1926 and 1940, and all now out of print. It contains no new material nor fresh treatment of the data. Over 4000 industrial and transport workers, including Services personnel, were subjected to a wide variety of psychological tests, and accident records for many more were examined. Two main conclusions emerged: that accident proneness was a relatively stable personality characteristic, and that a selective process that increased industrial proficiency would at the same time tend to decrease accident rate. Relationships between accident incidence and many other variables are reported, mainly in terms of weighted mean correlation coefficients; a small selection of tables and graphs from the original Reports is included. The pre-war work is regarded as unfinished, and as now in some ways dated; the author expresses the hope that others will wish to continue this line of research.

BORIS SEMEONOFF

English Social Differences. By T. H. PEAR. George Allen and Unwin. 18*s*.

Prof. Pear's interest in the contemporary scene is well known, and this work is an entertaining collection of his thoughts and observations upon class differences in eating habits, in clothes, in speech, in etiquette and a number of other spheres. Prof. Pear has been taken to task by some reviewers because his treatment of the subject is not sufficiently empirical, but his career in psychology, long as it happily has been, would have been far too short for all the topics in this book to be treated, in the fullest sense of the word, psychologically. Let us rather be thankful for a psychologist who is wise enough to draw our attention to problems which he does not himself pretend to have solved, and who is humane enough to do so in a manner which is highly diverting.

A. C. T. W. CURLE

American Indian and White Children: a Sociopsychological Investigation. By ROBERT J. HAVIGHURST and BERNICE L. NEUGARTEN. Chicago: University Press; Cambridge University Press. 1955. Pp. xiii+335. 37*s*. 6*d*. net.

This volume overflows with tables, giving in meticulous detail the results of an extensive series of tests administered to children of six Indian tribes and to a control group in a midwest city. Building upon the work of Piaget, new tests were worked out to study the moral and emotional development of children in such a way as to be applicable in cross-cultural investigations.

Two of the tests proved particularly useful and are commended by the authors to anthropologists as a systematic interviewing technique. In the Emotional Response Test children were asked to describe situations in which they were happy, sad, afraid, angry, ashamed, and to say what were the best and worst things that could happen to them. In the Moral Ideology Test children were asked to name specific forms of behaviour which they considered good or bad, and to name the source of authority for each. The recorded differences between the various Indians, whites, and New Zealand children (separately reported by Havighurst in an appendix) are in no way startling, but the results are such as to permit the authors to make general statements about the children of these communities.

MICHAEL BANTON

Growing up in the City. By JOHN BARRON MAYS. Liverpool: University Press. 1955. Pp. lx+216. 17*s*. 6*d*.

This is a first-class study of 'normal' delinquency, that is delinquency socially determined, connoting no personal instability in the delinquent. It brings a welcome new emphasis into a field overmuch given, in recent years, to equating delinquency with psychologically centred maladjustment. The eighty boys

whose experience Mr Mays records are almost without exception healthy boys who have grown up in a highly deficient and delinquent neighbourhood, and who have conformed to its patterns of behaviour. On a conservative estimate sixty-two were actually delinquent and most of the others probably so. Yet for all of them, with one possible exception, delinquency was or looked like being a passing phase, a kind of mild social measles. They had no feelings of guilt. They enjoyed the fruits of theft. They found it exciting—'it makes you sweat', as one boy said. There is a strong element of bravado. Once begun 'you have to keep going to prove you haven't got cold feet'. The main driving force is the impetus and approval of the gang, the small group of pals who go about together. Not all of the gangs are delinquent, but most of them are. They simply perpetuate the norms maintained by their predecessors, now their adult contemporaries. It is remarkable in the circumstances that a large number of boys avoid the more serious types of crime (petty shop-lifting from big stores and lorry-skipping are the more usual practices) and that most of them give it up between 13 and 16. It shows that the new non-delinquent phase, like its predecessor, is simply part of the process of growing up in that particular district. It shows also that even in a 'delinquent neighbourhood' the pressures to conform to the standards of the wider society are strong. The gang often keep together, but as they grow older they become more able to understand the risks they have been running, more reluctant to 'upset Mum', more capable of appreciating the feelings of the injured party.

One critical note. The author under-writes the view that young 'delinquents'—mild and transitory cases of the type he has observed—are primarily 'environmental'; while young 'criminals'—the more serious and incorrigible cases who constitute the hard core of the problem—are victims primarily of severe emotional disorder (caused usually by deficient parent/child relationships). This view is widely held. But the field of delinquency, or rather of the social and personal problems to which delinquency is a pointer, is vast and largely unexplored. Intensive investigation in this country has so far been conducted mainly by psychiatrists and clinical psychologists, who have naturally concentrated on personal disturbance. The further development of direct sociological and social-psychological inquiry on the lines initiated by Mr Mays may reveal a further type of hardened offender who is psychologically healthy; the 'social, gregarious, active and athletic boy', described by the late E. H. Sutherland, who takes on the impress of his delinquent neighbourhood more firmly than those boys who co-operated with Mr Mays to make this book. There is certainly room for inquiry, for example, into the composition of the strong informal groups to be found in a few approved schools and in many Borstals who effectively prohibit fraternization with staff. They are held together, it may be surmised, by leaders who have gone wrong by social rather than by psychological standards.

J. A. MACK

Oppression. A study in Social and Criminal Psychology. By TADEUSZ GRYGIER, with a foreword by HERMANN MANNHEIM. London: Routledge and Kegan Paul Ltd. 1954. Pp. xiv+362. 28*s.* net.

This is an account of an inquiry into the influence of oppression 'on human behaviour and on aggressive tendencies in particular'. Oppression is defined as a combination of 'five conditioning factors': lack of freedom, disregard of personality, isolation, insecurity, and frustration. In order to show that more intense oppression produces a greater tendency 'to direct aggression outwards', Dr Grygier used some of the T.A.T. material and Rosenzweig's Picture Frustration Study with groups of Polish Displaced Persons in the UNRRA camps in Germany. The groups were very carefully equated in all respects apart from the variable of 'background of oppression': former inmates of concentration camps were compared with former deportees to German industry and agriculture. A subsidiary investigation deals with delinquent Displaced Persons. The most striking finding here is the very high relative proportion of delinquents with the background of concentration camps.

This is an original, painstaking and courageous investigation, clearly written and fascinating. One may regret, however, that more evidence is not provided to justify the author's treatment of the population of the camps as 'homogeneous' at the time of his inquiry, apart from differences of background. This is possibly related to Dr Grygier's deep commitment to the 'criminal personality' view of delinquency. As he puts it himself, statements to the effect that delinquency may be an outcome of social norms and values 'explicitly contradict the whole basis of our study—the concept of antisocial personality, and the view, hitherto accepted, that the criminal personality breaks the rules imposed by any society or group'.

Dr Grygier's various statements about national character—Polish, Jewish, Russian or German—are on a level very different from the careful discussion of his equated samples of subjects. Here, as in the final chapter on the implications of the findings, the text sometimes tends to be an expression of

Dr Grygier's *Weltanschauung*, psychological and otherwise, rather than a contribution to knowledge. It is difficult to see how some of the author's generalizations on war and international co-operation, human nature, Apollonian and Dionysian societies, etc. are implied by, or even at times related to, the results of his research.

It should be stressed, however, that this does not detract from the value of the research itself, to which the major part of the book is devoted, along with a careful discussion and evaluation of all premises and conclusions.

H. TAJFEL

Integration of Religion and Psychiatry. By W. Earl Biddle. New York: Macmillan. 1955. Pp. xii+171. $3.75.

Psychology and Worship. By R. S. Lee. London: S.C.M. Press. 1955. Pp. 110. 7*s.* 6*d.*

Books on the relationship between psychology and religion are greatly needed, and the books by Dr Biddle and Dr Lee are therefore both welcome. The former author is an American psychiatrist, the latter an Anglican priest. It is unexpected, therefore, to find the psychiatrist re-writing Freud to fit Catholic doctrine; and to find the priest adopting a rigid Freudian position, Oedipus complex and all!

Much of Dr Biddle's book is devoted to expounding his 'new system of depth psychology'; and the chief interest in this book lies in the description of a system that can be happily integrated with Roman Catholicism. Dr Lee's system, on the other hand, is plain, unvarnished Freud, and the chief interest of his book lies in discovering how such a mechanistic system can be shown to be compatible with Christian doctrine and practice.

But as Owen has pointed out, 'to expose the psychological or sociological origin of a belief is not at all the same thing as proving it false'; and Dr Lee has no difficulty in accepting that the sacrament of Holy Communion has the same psychological basis as smoking, kissing and cannibalism. He quotes Freud's fanciful account of the horde father being killed and eaten by his sons, as an 'excellent parable' to describe the unconscious psychological forces underlying Holy Communion.

Although these two books are interesting, and well worth reading, it seems to the reviewer that an empirical psychological study of the nature of religious experience would be more rewarding than the attempted reconciliation of rival dogmas.

RALPH HETHERINGTON

Education and Psychology (Mental Testing Number, 1954, nos. 4–5). Calcutta: Elm Press, 1954. Pp. 180. 3*s.*

It is difficult to assess the purpose underlying the publication of this special number. Side by side with accounts of the standardization of group tests in various Indian languages are several vehement and unoriginal denunciations of mental testing, including a particularly irresponsible example emanating from an English university ('...intelligence tests have been widely used to "prove" the mental superiority of the white over the coloured peoples...'). Also included are personal contributions by each of the joint editors, of which one, a review of a book on the same general theme, contains this remarkable *volte-face*: 'We are in agreement with [the author's] fundamental objections to the theory and practice of intelligence testing. But we see no reason why [he] objects to its use if they help us in proper educational guidance.'

The scientific papers are uneven in quality, but fairly wide in scope (e.g. R. G. Chatterjee presents 'An Evaluation of Reaction Time, Time Sense and Cutaneous Sensitivity in Three Pick-pockets'), and the writers show an earnest desire to make use of and apply the methods and findings of Western research.

BORIS SEMEONOFF

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